

THE PSYCHOLOGICAL FACTOR IN
COLOUR CONTRAST

BY JOHN TENNANT

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1. INTRODUCTION.

THE recent work of Prof. Frank Allen of Manitoba is the occasion of and was the incentive to the following article. It has given me an opportunity both of criticizing some parts of Prof. Allen's work, more especially the article on "Reflex Visual Sensations and Colour Contrast," published in November 1923, and of reviewing the subject of Colour Contrast or Simultaneous Induction in some, at least, of its aspects.

I hope in so doing I shall not be thought to underrate the value of Prof. Allen's remarkable contributions to the subject. He has made new discoveries as well as shed a decisive light on old ones. He has produced

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strong evidence for the fundamental correctness of the Young-Helmholtz theory of vision, he has shown that the third fundamental sensation is almost certainly violet and not blue. He has shown the varying effect upon observations made by one eye, of light or dark sensations in the other, and thus throws some doubt on the accuracy for ordinary daylight of many older observations made in the dark room. Above all, his application of the Flicker method to Auditory, Gustatory and Tactile Sensations has shown that 'reflex effects,' or, to use the old term, 'simultaneous induction,' is not confined to visual sensations, but, at any rate in cases which can be covered by the term fatigue, is a general property of nerve tissue, and thus probably compatible with any theory of vision which suits the other visual phenomena.

His paper on Colour Contrast does not appear to me a very decisive contribution to the special difficulties of a subject in which experiments using fatigue or long fixation can throw comparatively little light.

2. HELMHOLTZ ON COLOUR CONTRAST.

The controversy may perhaps be said to have begun with the publication of Helmholtz's explanation, and is not yet quite at an end, for Dr Edridge Green¹ accounts by a similar explanation for the well known coloured shadow experiment, and von Kries, without absolutely rejecting the direct physiological explanation of Hering, adopts in the main a modified form of Helmholtz's theory.

Helmholtz's explanation was a purely psychological one. In introducing his remarks he used the following illustration: "By the side of a big fellow a man of medium size looks small because we see clearly that there are larger men, but not that there are also smaller ones²." The illustration never appeared to me convincing. I pointed out in a paper read before the British Association in 1886 that in the illustration there is an obvious judgment from one thing seen to a different thing seen, from apparent size determined by the relation of two visual angles to real size as measured. But in comparing colours we are dealing with an immediate deliverance of sense, and there is at first sight no reason why we should suppose such deliverance to depend upon a judgment in the way indicated. However³, the psychological theory of simultaneous contrast cannot be dismissed off-hand, particularly since Helmholtz supported his opinion by many experiments. It is in these experiments

¹ Edridge Green, *Physiology of Vision*, p. 232.

² Helmholtz, *Phys. Optics*, Centenary ed. II, 270.

³ *Nature*, XXI, 144.

that the strength of the theory lies, but I reserve my criticism of them until the evidence for a physiological explanation has been examined and criticized.

The main arguments for a physiological explanation fall into two classes:

1. Arguments derived from the study of after images, or at any rate from phenomena occurring after considerable fixation.
2. Arguments from the measurability of contrast phenomena.

3. HERING'S CRITICISMS.

Hering, the most important critic of Helmholtz's theory, explored both these lines of argument, but historically his first attack on Helmholtz's explanation was a tract, *Zur Lehre vom Lichtsinne*, republishing a paper read before the Imperial Academy of Science of Vienna in 1873¹.

In this tract Hering describes (*inter alia*) the result of the fixation, say for 60 seconds, of a white square upon a dark ground. The after image is dark on a lighter ground, but is surrounded by a bright halo, of a brilliance so great, that it makes a psychological explanation of the whole appearance quite impossible². He adds to this many ingeniously devised experiments of the same nature which rebut all the endeavours of his opponents, and especially the use made by Helmholtz of the *Eigenlicht*, the unstimulated activity of the retina for the purpose.

It is impossible to read this paper, carefully repeating the experiments, and not to be convinced that the phenomena therein described must receive a physiological explanation³. Whether this is decisive of the whole controversy, or whether any experiments under the first head of argument can be so, is another matter.

Hering's experiments were all made in daylight. They are all easy to repeat and are explicable by his theory. To explain them on purely psychological grounds seems impossible. In this particular tract the contrasts are all between black and white, or between darker and lighter surfaces. Prof. von Kries, who may be considered in general a supporter of Helmholtz⁴, seems to yield the point in the case of black and white, but to doubt whether contrast of colours can ever be explained on purely physiological grounds.

¹ *Lehre vom Lichtsinne*, Wien, 1878, pp. 5-7.

² *Ibid.* p. 14.

³ See McDougall, *Mind*, x, N.S. No. 37, 52.

⁴ Von Kries, *Allgemeine Sinnesphysiologie*, Ch. ix.

4. McDougall's EXPERIMENTS.

The next papers I shall refer to are a series of articles published in *Mind* in 1901¹.

These important and, as Sir J. H. Parsons says, these much neglected articles, were an endeavour founded on a brilliant series of experiments on 'fatigue' and after images, to reconcile the Young-Helmholtz theory of vision with the phenomena of colour contrast and successive induction, and to show the inadequacy of Hering's theory to account for all the facts.

A good account of these papers, and of the physiological hypothesis advanced by McDougall, is to be found in Parsons' *Colour Vision*².

In these papers McDougall, using a much wider range of observation than Hering, showed that Hering's theory was especially inadequate to explain the positive after images which do not occur in daylight with the moderate illumination used by Hering. He proved that in some cases, a light falling on one part of the retina can inhibit response in another part of the retina which has been weakened by 'fatigue.' He explained the going and coming of the separate parts of the after image by this principle of inhibition, and he compared these phenomena with the well-known case of the competition of different images on the two retinas.

As a result of these experiments he adopted a theory originally suggested by Rollet to account for immediate colour contrast.

Accepting the hypothesis of three independent colours, red, green and blue, he showed that as the grey of the contrasting ground contains all these colours, the juxtaposition, say of a bright red, might well inhibit the red factor in the adjoining grey and so produce upon the grey a blue-green shade.

Among McDougall's experiments supporting this, one may specially be mentioned³. In the dark room, a white sheet was placed dimly lit by diffused light. At a distance of 30 cm. on the wall of the dark room facing the light was a circle 5 cm. in diameter which, by means of a photographic shutter, could be suddenly illuminated with bright red light. From a distance of 70 cm., a point midway between the circle and the sheet was fixated for *not less* than 15 seconds, and the bright red

¹ *Mind*, x, N.S. No. 37, 52; No. 38, 210; and No. 39, 347.

² Parsons, *Colour Vision*, 1st ed. pp. 104-282.

³ *Op. cit.* No. 39, Observation 38, p. 350.

circle was suddenly lit up. Immediately the white square became a blue-green of good saturation somewhat varying in tint. The square remained blue-green so long as the red circle was visible, but when the shutter was closed it lost all colour and became slightly tinged with red.

Prof. McDougall seems to have established as a fact the action of inhibition in this and similar cases. It is true that 'fatigue' takes place in all these experiments, but Rollet's theory seems to give a reasonable explanation of colour contrast, once the natural inference from comparative experiments is accepted, and to remove the reproach under which Young's theory has suffered of failing to account for the phenomena.

The paper deals at length with all the appearances that follow long fixation, and, adopting a chemical theory of the outflow of red-exciting, green-exciting, and blue-exciting substances from a stimulated part of the retina under steady fixation into the neighbouring portions, gives a reasonable account of the origin of the appearances.

Any one who has fixated for some time a red disc, say on a medium grey ground, and has noticed the gradual diffusion of the red colour over the ground, will feel that McDougall's primary hypothesis is a very natural one.

It is, however, possible to admit a physiological explanation when 'fatigue' occurs, and to deny it in the typical experiments from which the controversy began, though Helmholtz did not take that line of reply to Hering's paper.

5. ALLEN'S EXPERIMENTS.

I now turn to Prof. Allen's experiments. A full and appreciative account of these is given in Sir J. H. Parsons' *Introduction to the Theory of Perception*¹.

At the moment I am particularly concerned with two papers published in the *Journal of the Optical Society of America* on the "Reflex origin of colour contrast"; the first and most important in Vol. VII, No. 11 and the other in Vol. IX, No. 4.

The actual method employed in the first of these papers illustrates the general nature of the work, and was as follows:

Prof. Allen² first obtained a normal persistency curve by the Flicker method, using a narrow rectangular strip of an arc spectrum seen by the left half of the right eye, and fixating a particle on the edge of the shutter

¹ Parsons, *Introduction to the Theory of Perception*, Ch. XII, p. 196.

² *Op. cit.* VII, 915.

covering the other half of the eye, on the right side of a patch of colour. This normal persistency curve, which is the inverse of the luminosity curve, was thus obtained by slightly indirect vision, but agreed perfectly with the similar curves obtained by direct vision in earlier experiments. The shutter was then transferred to the left half of the eye, and the right half of the eye was fatigued by fixating a patch from a second arc spectrum, again focusing upon a particle on the edge of the dividing shutter. The effects of this fatigue on the normal curve could thus be investigated. I gather from a former paper¹ which traced the effects of fatigue of one eye on the other eye, that the arc spectrum was so bright that the colours in the middle part of the spectrum "approached whiteness in appearance²," and that fixation was continued for two or three minutes. By the method used, 'fatigue' could be confined to one retinal area, and the induced effects, if any, transferred to an adjoining area could be measured.

Prof. Allen's results are summarized in Sir J. H. Parsons' treatise³ before mentioned in the following manner:

1. Stimulation of one area of retina with light causes:
 - (a) Direct effects upon that area.
 - (b) Induced effects in all other areas, *i.e.* both in the same eye and the other eye.
2. Direct effects—with both eyes in a condition of moderate photopia:
 - (a) Stimulation with white light depresses the activity throughout the spectrum.
 - (β) Shading the eye also depresses the activity throughout the spectrum.
 - (γ) Stimulation with any given wave-length depresses the sensitivity to this region of the spectrum and enhances it for the complementary colour.
 - (δ) With any given wave-length the depressions and enhancements show three curves affecting respectively the red, green and violet.
 - (ε) Stimulation with the equilibrium colours $660\mu\mu$, $570\mu\mu$, $520\mu\mu$, $505\mu\mu$, $480\mu\mu$, $425\mu\mu$ has no apparent effect upon the sensitivity or the visual apparatus.

¹ *J.O.S.A.* VII, No. 8, 585.

² *Ibid.*

³ Parsons, *op. cit.* Ch. XII, p. 207.

3. Induced effects: With both eyes in a condition of moderate photopia, the effects on the unexcited areas of the same and the opposite retina are:

- (α) Stimulation by white light enhances the sensitivity throughout the spectrum.
- (β) Shading an area or an eye depresses the activity throughout the spectrum.
- (γ) Stimulation with any given wave-length enhances the sensitivity throughout the spectrum, the enhancement being most marked in every case in the red, green and violet.
- (δ) Stimulation with any given wave-length enhances the sensitivity most for the complementary colour.
- (ϵ) Stimulation with the equilibrium colours has no apparent effect.

6. CRITICISM OF ALLEN'S RESULTS.

In this paper, I am chiefly concerned with the induced effects under the head 3 (δ), but I make one general criticism.

It appears to me that the brilliance of the illumination used, and the long fixation, introduced complications which render generalizations of a quantitative nature for lower illumination and shorter fixation somewhat uncertain, and may seriously affect the conclusions drawn¹.

A more recent paper, using varied and lower illuminations, has given valuable results concordant on the whole with those already arrived at, but the long fixation, which is inherent in the method, makes it scarcely possible to apply the conclusions derived from these laborious investigations to any visual experiences in which fatigue is not a factor.

An experiment of Prof. McDougall points to the danger of relying for quantitative results on such long fixation as two or three minutes, especially with the brightness used in the middle parts of the spectrum. Prof. McDougall² laid a disc of red paper on a grey ground in strong lamplight. After two and a half minutes' steady fixation, the red actually passed through yellow to a bright blue-green, changing back after a few seconds to red and then to green, with several alternations. Evidently such prolonged fixation has very complicated effects, and such complication will probably not be confined to the direct effects, but will affect the induced effects in adjoining areas. Taking this into account and

¹ *J.O.S.A.* XIII, No. 4.

² *Mind*, x, N.S. No. 38, 221.

other experiments in these admirable papers in *Mind*, it seems somewhat dangerous to make quantitative inferences from the experiments of Allen to cases of much shorter fixation, and to transfer them to the typical case of immediate and instantaneous colour contrast seems quite inadmissible.

To turn to the specific point of simultaneous contrast. What is the exact type of experience which Prof. Allen has analysed by the spectrometer?

The following is a fairly close parallel. Take a square of ruby glass and divide it into two by a black card, point to a white cloud and fixate the middle point of the card-edge with the right eye for two or three minutes. Then cover the ruby part of the glass and notice the after image, say, on a grey ground.

Now in such an experiment the complementary colour, if seen at all, is seen only in the first few seconds. If I use a small square patch of grey paper and illuminate it from behind, I do see it at first, but it rapidly vanishes and in both cases the red colour begins to invade the dark part, and a rosy flush, deepening as fixation continues, covers the neutral part of the glass.

It would seem, from these experiences, that we should expect the induced effects in Allen's paper to be similar and not complementary to the fatiguing colour. When we look carefully at the figures in Prof. Allen's paper, it appears that the conclusion 3 (8) is not legitimately drawn from them. In some of the figures it is the more probable inference from the facts, but three of the figures show not the reinforcement of the complementary but of the inducing colours in the other half of the eye.

Very clear is this in the case of $\cdot 588$ and of $\cdot 410$ and fairly evident in the case of $\cdot 676^1$. It does not appear to me that these results of long fixation throw any but indirect light upon the main subject of the present paper.

The conclusion I arrive at is that no demonstration that colour contrast has a physiological origin can be derived from experiments of the 'fatigue' type, but Rollet's theory with McDougall's experiments gives a persuasive explanation of the result, and Prof. Allen's demonstration in his long series of experiments on the various senses that induced effects are a general property of nerve tissue, throws the burden of proof upon any who still uphold a purely psychological explanation of the induced phenomena.

¹ See *J.O.S.A.* VII, 919, 926, 917. See also reflex curves in Parsons' *Introduction to the Theory of Perception*, p. 201.

7. FURTHER EXPERIMENTS ON THE OBJECTIVE CHARACTER OF COLOUR CONTRAST.

The second head of proof that the simultaneous contrast effects are objective is derived, to put it broadly, from the fact that they are measurable¹. Hering and others have made many measures chiefly by means of the colour top and their conclusions are summarized by Parsons².

I have myself made similar experiments with a clockwork colour top having two discs side by side. The method I used was to arrange a grey strip on one disc on a black ground and a grey strip of like tint and breadth on a coloured ground, and then see either how much of the ground colour must be added to this disc to match the first, or what colour has to be added to the first to match the latter. I have not made a long series of systematic experiments, but in a general way I have verified Hering's conclusions, especially that the more saturated the coloured disc the stronger is the induced effect.

The objection which can be made to these experiments depends on the difficulty of excluding successive contrast. It must be admitted that the danger is real. If I place a narrow strip of saturated colour on a grey ground, a single second's glance, followed by a rapid removal of the coloured strip, shows a complementary coloured strip which fades with great rapidity. But in the series of experiments, by Hering, Hess, Pretori and others, great pains were taken to exclude successive contrast, and the results seem to be reliable³.

It is not, however, necessary to measure the effects of contrast in order to demonstrate the physiological explanation. It is sufficient to show that in the same field of vision in close proximity, and if necessary with strict fixation, two grey strips of the same colour appear different with different surroundings.

A single page of pictures in Hering's *Lehre vom Lichtsinne*, Tafel II, opposite p. 116, should suffice to carry conviction as regards the contrast of black and white.

The following modification of the experiment on p. 117 of that work is to me extraordinarily convincing and easy to verify.

Punch a hole in a fairly thick white card, and on a table facing a

¹ Parsons, *Colour Vision*, 1st ed. pp. 107, 128.

² *Ibid.* Sect. III, pp. 263, 266.

³ For another method of measurement not dependent on rotation, see Hering, *Grundzüge der Lehre vom Lichtsinne*, p. 126.

window, lit by diffused daylight, place a similar piece of white card slightly inclined away from the light. Then, holding the punched card parallel to the window, in such a position that a small portion of the fixed card is seen through the hole, slowly revolve it from a position fully facing the light to a position quite turned away from it. The changes from dark to light of the visible portion of the fixed card are astonishing. It is instructive to vary the operation by keeping the upper card fixed and revolving the lower. In the former case the light which the fixed card sends to the eye is the same throughout, and the apparent changes due wholly to induced effects; in the latter case the light itself varies, and yet the changes, though their range is somewhat greater, are hardly more remarkable.

Now, using a smaller hole, say a quarter of an inch in diameter, take two white squares and make the holes in each an inch from the side, so that when the squares are placed side by side the holes are two inches apart and easily fixed in the same field of view. Then, in repeating the former experiment, revolve the upper squares in opposite directions. There is seen in the same field of view two circular patches, one nearly black, the other nearly white, and yet both sending the same objective light to the eye. There is here no question of merely judging a relation, but a direct comparison of the patches themselves, and the conclusion is surely irresistible.

The colour experiment I suggest is, I believe, new, and as it makes use of coloured shadows is specially interesting.

I filled two electric torches with Wratten's gelatine discs. One was covered with a red disc (Wratten's filter A.) which excludes all the spectrum of shorter wave-length than $600\mu\mu$. The other was charged with a green.

In a room lit by not very strong diffused daylight, and on a white ground, I placed a cylinder, say two-thirds of an inch in diameter. The electric torches, in order to narrow the beam of light, were placed in tubes with blackened insides and with the bulb about one inch from the end. They were then held in a position at right angles to one another and directed at the cylinder. Then the light was turned on. The shadow of the red torch was blue-green, and of the green violet. Both were in the field of vision at once, both sent the same light to the eye, viz. the dim daylight of the room, and yet they looked absolutely different. Moreover, this is so for a momentary exposure.

These experiments demonstrate the reality of a physiological factor in simultaneous colour contrast. It probably depends, as McDougall has

explained, on the principle of inhibition. It is, however, possible that reflex effects of a positive nature, such as Allen has shown to exist in cases of long fixation, may also affect the results.

8. PSYCHOLOGICAL FACTORS IN COLOUR CONTRAST.

The question then arises, how are we to explain the adherence of a brilliant intellect, such as Helmholtz's and of many lesser men, to the purely psychological explanation? Why, for instance, in the ordinary course of things do not people notice the contrast effects more commonly, and why does it need special arrangements, coloured glasses, coloured shadows, spinning tops, tissue paper covers, to reveal them?

There are, I think, two factors of a psychological character which explain this.

The first was pointed out in a paper I read before the British Association in 1886. We are not in practical life concerned with the varying deliverances of sense, except so far as they interpret for us the outside world which vitally concerns us. What matters to us is not the play of light and shadow but the local colour, the appearances which betray and reveal what the object is¹. For the lower animals it must have been a vital necessity to recognize local colour so as to identify their foes or their prey, and at any rate in the long course of human evolution this has been true. We have been trained as a race by rapid inference under varying circumstances to recognize the normal colour of things seen², and quite unconsciously our judgments concern ourselves with that.

Helmholtz has stated this law of vision as follows: "In our observations with the sense of vision we always start by forming a judgment about the colours of bodies by eliminating the difference of illuminations by which a body is revealed to us."

Anyone who possesses a series of grey papers of various tone has at hand a simple and striking means of illustrating this law of vision. A subdued grey, close to the single window of a room, may send more light to the eye than a white paper at a distance from it, and yet the one will appear white and the other grey; and the light greys can be picked out from the dark, irrespective of whether, in their particular relation to the light, the dark one sends more actual light to the eye than the lighter one. The true luminosity relations can be ascertained either by isolating each paper from its surroundings by the use of a tube, or, if you can get them

¹ Von Kries, *Allgemeine Sinnesphysiologie*, Ch. IX, p. 260.

² Hering has called this "die Konstanz der Sehdinge." I shall refer to it as the principle of the recognition of normal colour, see von Kries, *Allgemeine Sinnesphysiologie*, p. 260.

side by side in the same line of vision, by closing one eye and confining the attention to the surfaces compared.

Hering has shown, by an experiment in which a white wall opposite the single window of a room appears darker than a grey paper near the window, that this recognition of normal colour takes place under the simplest conceivable circumstance, and if Helmholtz differs at all, it is only in making our judgment more deductive and less instinctive than it really is.

In the experiment with the greys we actually see, not what pure untutored vision would affirm, but what the totality of our living experience informs us. Thus the psychological factor is real, but usually works, not as Helmholtz believed, to make us see colours which have no physiological or objective foundation, but to make us ignore colours which are not permanent property of the objects around us.

The second factor of a psychological nature that affects our perception of tint is this. The very fact that we recognize a grey, a white, a coloured object under various illuminations, means that our standard of colour is not a very nice one. This is especially the case with greys, much less so with saturated colours. I have a series of grey discs, procured many years ago from Hering's makers, some of which seem to me too blue, and others at times too brown. The spectrum under scotopic conditions seems to me pure grey, but others think it has a bluish tinge, nor can I make up my mind as to which of the greys in the French colour chart is pure neutral grey. If one compares a mixture of two complementary colours on the colour top with a mixture of black and white, it is surprising how the delicately tinted colour disc affects our judgment, the one certain judgment being a relative one, that this is on the purple side of grey and that on the green, or *vice versa*. Hence Peddie is quite right in saying that in the case of white or grey we have no very exact standard to guide us¹, but he has hardly indicated the true factor when he adds that we regard the brightest light as white. The brightest light, when the sun looms red through mist, is red, but white paper looks white, and grey paper grey, under such illumination, quite irrespective of the actual physical rays sent to the eye. This result is possibly largely of a physiological character, but I think the top experiences referred to show that there is a psychological factor as well, and that in comparing two greys the relative judgment is much easier and more certain than the absolute one. We can say this grey is on the blue side of that, much more confidently than we can affirm that the one is tinted, the other pure².

¹ Peddie, *Colour Vision*, p. 17. ² See Helmholtz's *Phys. Optics*, Centenary ed. II, 2.

9. DISCUSSION OF OTHER CLASSICAL EXPERIMENTS.

I shall now consider the classical experiments, and I turn first to the coloured shadow experiment which I shall discuss at some length.

(a) Helmholtz's coloured shadow experiment.

This consists essentially in comparing the two shadows cast in a dimly lighted room by diffused daylight, and a yellow illuminant on white paper. The eyes should be previously covered, and one spot fixated to avoid the effect of successive contrast. The shadow cast by daylight and lit by the candle is yellow, as it should be, that cast by daylight is not grey, but blue. A cloudy day should be chosen, as shadows lit by a blue sky are naturally blue.

In making the experiment Helmholtz used a blackened tube, fixated a spot in the candle shadow so that only the candle shadow appeared in the tube, and noted its unaltered character whether the candle be lit or not. He said¹:

On the other hand, if the tube is held to the eye and directed so that a part of the field surveyed is illuminated by the red-yellow light of the candle, the shadow from the candle again becomes blue. When the blue has become really intense let the tube again be pointed so that nothing but the subjective blue is in the visual field. The blue now persists whether the candle is allowed to shine on the rest of the paper or whether it is screened, which of course, so far as the observer is concerned, amounts to the same thing because under these conditions he is not aware of it at all. The blue colour in such case is so constant that Osam has concluded from similar experiments that it is objective. This opinion is easily refuted by the fact that the blue colour persists even when the candle is extinguished. But at the moment when the black tube is removed from the eye the subjective blue disappears also because it is then recognised to be identical with the white that occupies the rest of the visual field. No experiment shows more impressively or more clearly the influence of judgment on our determination of colour. As the result of the contrast whether it be successive or simultaneous, once the judgment has been formed that the colour in the shadow of the candle light is blue, the colour continues to appear blue although the circumstances that led to the decision have ceased to exist; until the black tube is removed so as to enable us to make a new comparison with other colours and to form a different judgment in the light of new facts.

(b) Hering's modification.

Hering repeated this experiment² of Helmholtz with elaborate precautions. His tube was split in half in such a way that it could be made to fly open by touching a spring. He used a lamp that could be extinguished or turned on at will. He made the following preliminary

¹ Helmholtz, *Phys. Optics*, Centenary ed. II, 272.

² *Archiv für Phys. Pfluge*, XL, 172.

experiments. He first gazed at the white field with the lamp out; then lighting the lamp, he noted the yellow tint added to the field which gradually faded under fixation. Putting out the lamp, a blue after image appeared, whose intensity depended on the length of the former fixation but was visible even after a second and did not depend on the use of the tube. He then, using the tube with lamp out, fixated the white ground for 20, 40, or 60 seconds, and noted the after image which sometimes showed weak coloration and needed for distinctness careful and steady fixation. Now lighting the lamp, he fixated a portion, and putting out the lamp, noted the blue after image. Then touching the spring, he opened up the tube and the image lost blue, the colour becoming fainter.

Hering's explanation of this is difficult, but it is easily understood from Prof. Allen's experiment. It turns on the effect of the presence or absence of the tube, and takes an important place in Prof. Allen's criticism of Helmholtz.

After these preliminaries Hering, using the tube, fixated the edge of the candle shadow, and saw an after image half blue and half grey or even yellow on moving the field of view wholly into the candle shadow. Then again putting out the lamp and opening up the tube, he saw an image partly blue and partly grey, thus proving that the prolonged vision of blue was the result of successive contrast.

Hering pointed out that the fact that the blue in Helmholtz's experiment is described as developing strength with time, showed that fixation must have been imperfect, as colour contrast was greatest in the first instant and diminished, and could even be reversed by long and steady fixation.

Allen has also discussed Helmholtz's coloured shadow experiment and agrees with Hering that the continuation of the blue image is due to successive contrast.

(c) *Edridge Green's coloured shadow experiment.*

Edridge Green¹ has, however, devised an experiment with a rather unexpected result which at first sight strongly suggests a psychological explanation of the coloured shadow experiment, and which at any rate demands careful examination.

He says:

If in repeating the experiment the opaque object be placed upon a dull black surface and two pieces of white paper be placed on this surface for the shadows, care being taken that these pieces of paper are the exact size of, or smaller than the

¹ Edridge Green, *Physiology of Vision*, p. 232.

shadows, these will appear blue and yellow as before. If we now place a small dot on the paper on which there is a blue shadow, and having covered one eye keep the other rigidly directed at this black spot while an opaque object is placed in front of the candle so that it no longer illuminates the paper or throws a shadow, both pieces of paper will appear white being illuminated only by white daylight. The eye being still rigidly directed on the spot on the paper, the opaque object is removed so that the candle again throws a shadow on the paper. The shadow thrown by daylight immediately appears yellow of greater saturation than before, but the shadow thrown by the candle appears white as before, and without the faintest trace of blue or blue colour. The conditions are everyway favourable to the development of the blue colour but none appears, because the observer has been able to form a correct estimate of white. If the blue colour were a real subjective coloration caused by the yellow, blue should appear on the shadow from the candle.

In repeating this experiment, I made some modifications which gave interesting results.

(d) A new form of the shadow experiment.

I used, as the black cover, a black flock paper, reflecting the least light possible, and for a white ground a piece of pure white card. The flock was about a yard square and completely covered the table used. In all the experiments the position of the light and the opening of the shutter were so arranged as to give the richest and fullest effects with the white ground. A cylinder caused the shadows, and in all cases it stood on a circular card of black flock which hid the junction of the shadows. I prepared two white papers, as in Green's experiment, marked in the centre with a spot of ink for fixation. Besides these, I prepared two cards covered with black flock and long enough and broad enough to cover the shadows completely.

I made some preliminary experiments, using the white ground. I covered the two shadows with the black cards, and, after resting the eyes and fixating with one eye either the card covering the candle shadow or a point between the two shadows, I sharply removed both cards and noted the colour of the blue shadow. Replacing the black cards and fixating as before, I now removed only the card covering the candle shadow. I could see no difference whatever in the blue colour, nor did it matter to the colour whether the daylight shadow was covered or uncovered. In the classical experiment, then, it is the ground colour that matters, and the presence or absence of the daylight shadow is a matter of indifference. In these experiments the eye should be rested from time to time, and long fixation of the blue shadow which causes fading, avoided.

I now fixated the blue shadow steadily and then cut off the light both from the ground and my eyes. I saw a pale yellow after image of the candle shadow surrounded by a rather rich blue ground. The

after image of the daylight shadow was too dark to show definite colour.

I now used the black ground and, with candle lit, placed the white paper on the shadows, covering them at first with the black cards. If I accustomed my eyes to the doubly lit room, and then removed the black cards, the candle shadow appeared a faint blue but nothing like so saturated as the same shadow when the ground was white. If now I rested my eyes and, keeping the light from my eyes, I fixated the candle shadow as in Edridge Green's experiment, the paper looked white, but I could not convince myself that the daylight shadow appeared more saturated. If I again shaded the light and now fixated not the white paper but a black flock card covering it, and then sharply removed the black cards from both shadows, the candle shadow was white as before, though in this case I had not accustomed myself to the white of the cover. Moreover, when, after letting my eyes wander, the candle shadow appeared blue, it hardly seemed to alter at all whether the daylight shadow was or was not covered by the black card, again pointing to the fact that some other factor than the yellow shadow was at work in causing the blue colour.

On the other hand, if I used instead of the white cover a quite *small* rectangular white stripe, and placed it across the candle shadow so that one part projected beyond the edge of the shadow, then, though I covered the daylight shadow with the black card, and with candle lit fixated the black card over the candle shadow and then sharply removed it, the blue of the shadow was present in spite of the small size of the lighted ground near it.

These experiments with a candle were made in a room rather darkened by plane trees, and the blue of the shadow when the black ground was used was always faint.

I made similar experiments in a room well lighted, on a cloudy day, using an electric lamp with a white shade which could be turned on or off at pleasure. In this case the blue tint of the candle shadow when the black ground was used was more evident than in the candle series, but it still bore no comparison to the colour of the same shadow when the white ground was used. It was also in this experiment much more difficult, by fixating the candle shadow with lamp out and then getting the light turned up, to get rid of the blue, and indeed in the small rectangular stripe modification it was impossible if I was facing the light. In all cases the rectangular stripe within the shadow appeared blue and the projecting part white.

If, however, I did not face the light but stood by it, carefully shading my eyes when I turned it up, and then, with the light out, fixated the transverse rectangular stripe and turned on the light, I then saw the part within the shadow white and the projecting piece lit by the double light of lamp and daylight a good yellow. The daylight shadow was covered by the black card in both these experiments.

(e) *Discussion of results.*

It seems clear from these facts that some other factor than pure physiological contrast is operative, and clear also that it is the doubly lit ground that matters and not the other shadow. I say another factor because the effect on the production of blue of so very small a portion of the doubly lit ground as the stripe is undoubted, and in the last experiment made was shown not to operate constantly but to depend on suggestion.

Edridge Green's explanation might possibly be good for very faint colours, slightly differing from white, and might possibly apply to the last experiment recorded as between the two whites of the singly lit and doubly lit ground, but, used as a general explanation of the classic experiments with shadows or similar strong effects, it fails to account for the fact that contrast colours are always complementary, or nearly so, to the surrounding ground. Not only has Rood shown that our memory for full colours is good, but if we are dependent on estimates of the ground colour there seems no reason why the contrasting effect should not sometimes resemble the ground. Take the case of a grey strip on a red ground. If we correctly estimate the saturation and tint of the ground the strip should look grey, if we overestimate its saturation and colour the strip should look blue-green, if we underestimate its saturation the grey should, according to Edridge Green, look reddish. I cannot think that estimates by memory play any considerable part in the phenomena, and the above experiments show that the daylight shadow, at any rate, is not an operative factor.

In all these experiments, with a room partly lit by daylight and partly by yellow light, except with very special precautions such as those used in the final experiment, we actually see the ground white. It seems to be a direct perception and not an estimate, and seems to point to that adaptation of the eye to changes in general lighting which I have already insisted is a consequence of the necessity men and animals are under of recognizing colours as objectively the same under varying conditions of illumination.

Is it then a physiological or a psychological effect? Are the sensory

impulses so directly modified by the general lighting, that they produce the same effect as under fatigue; or are we to explain the result not in terms of sensation but of direct perception as, without doubt, the recognition of the darkness of the different greys is to be explained in Hering's experiment? It is not, it seems to me, psychological in the sense that we consciously compare the white of the ground with our remembrance of a standard white, but it is a perceptual reaction to the whole surroundings in which we are placed, and this reaction is explained and necessitated by the evolution of mankind which has made the recognition of the normal colour of objects a vital necessity. This does not disprove the simultaneous action of the physiological factor, which, in the very case of coloured shadows, the experiment with two electric torches proves to be operative.

The results of the last experiment render me doubtful if Hering's explanation of Helmholtz's results is completely satisfactory. It seems at least possible that the suggestion that the shadow is blue continues to operate when the light is put out, and that the sudden change to white is explicable, as von Kries¹ holds, by those effects on our perception which he calls 'Umschaltungen.' For these, and for the general treatment of psychological effects on perception, I must refer to his valuable chapter. My chief difference from him is that, while he admits the simple physiological factor in the case of black and white, he doubts its operation with colour. This error, as I regard it, seems to me adversely to affect his judgment in several places.

(f) *Ragona Scina experiment.*

I pass on to discuss at some length another of the classical experiments, that of Ragona Scina, as I think it throws, when carefully considered and varied, a valuable light on the whole controversy. It consists essentially of two square boards at right angles, which I shall call the floor and the wall. Both are covered with white paper, and a plate of coloured glass divides equally the angle between them. The wall with the glass and floor in front of it is placed facing a window. On the floor is a pattern made in black and white, and on the wall the same pattern with the black and white interchanged. The patterns are so adjusted that the reflection in the glass of the wall pattern coincides with the pattern on the floor transmitted through it. The white light reflected from the glass when the glass is not too thin or transparent causes a grey pattern to replace the black of the floor, while the white parts of the pattern take a deep colour from passing through the glass. The general white of the floor

¹ Von Kries, *op. cit.* For the shadows see p. 276.

when opposed by the white of the wall causes the colour of the ground to be diluted with white.

The grey parts seem tinged with a colour complementary to that of the glass. Thus when the glass is green, the grey looks a pale rose or violet, and when the glass is blue, yellowish. If the experiment is made without due care, by allowing the gaze to wander over the pattern, the contrast colours are fairly vivid, though never, I think, quite so unmistakable as the coloured shadows in the last experiment, but in this case successive contrast is operative, and great care is needed to make the experiment accurately. It is important to adopt Hering's method, and to interpose in front of the pattern a black screen. This can be fixated, removed, and replaced after a few seconds. When this is done, the complementary colour is much less vivid, successive contrast having been excluded.

(g) *Helmholtz's modifications.*

Helmholtz notes that there must not be too much difference in luminosity between the grey pattern and the ground¹, and he recommends that when the coloured glass transmits so much light as to show the dark parts of the pattern too dark, it should be covered with white tissue paper. I have found that in such cases it is effective to cover the coloured glass with a colourless one, thus increasing the number of surfaces that transmit white light to the eye. Helmholtz, after describing the arrangements necessary for the experiment, continues thus:

Now if the observer will find a grey paper of exactly the same colour as that in which the spot 'e' (*i.e.* the dark part of the floor pattern) would appear to him without contrast, and hold a small piece of it over the coloured plate so as to hide half of the spot 'e', this little piece of paper will not appear in the complementary colour at all or even show any suspicion of it; and the moment the colour of the spot 'e' is compared with it and seen to be the same, the complementary colour of 'e' disappears also and changes into a simple grey.

(h) *Hering's criticism.*

Hering² has published a careful criticism of this experiment. He used, instead of the small square patch 'e' used by Helmholtz, a pattern of black and white concentric rings. He enclosed the whole apparatus in a dark-lined box, with one side open to the light, using the side facing the wall. He covered this with a lid in which was a circular hole, through which the pattern was seen and which completely hid the edges of the plate. This aperture was covered, a spot of the pattern fixed, and the cover removed. Even children saw the contrast colour. Then, like

¹ Helmholtz, *Phys. Optics*, Centenary ed. II, 283.

² *Archiv für Phys. Pfluge*, XII.

Helmholtz, Hering brought a selected grey disc above the glass matching the grey of the pattern. He agrees with Helmholtz that both look alike, but affirms that the grey disc as well as the pattern show the complementary colour of the ground.

I do not need Hering's elaborate arrangements to agree with this decision. I see both the grey paper and the pattern reflected through the glass alike, showing the complementary colour of the ground.

It is to be noted that Helmholtz himself, in the passage I have previously quoted, admits and indeed insists that there is a tendency to see objects in their normal colour in spite of strange lighting and other illuminative effects, and Hering's arrangements, by excluding the presence of other subjects, may be adapted to counteract this tendency, and so make it easier to recognize contrast effects, if such exist.

Thus the difference between the two is this:

Both agree that the grey paper matches the pattern, but while Hering insists that the contrast colour is a real effect due to a physiological cause independent of any act of judgment conscious or unconscious, and would, I suppose, explain Helmholtz's failure to recognize it, by the tendency which Helmholtz himself admits in some cases to ignore colour resulting from position and special circumstances, and to see only the normal colour of the object, Helmholtz accepts the pure grey of the paper, as given directly in sensation, and gives an explanation of the colour seen through the glass which appears to me more ingenious than convincing. He says:

The observer¹ decides that the black spot on the horizontal surface is pink-red, but he also decides that this spot as well as the entire surface with its pink-red colour is seen through the green glass and that the green colour given by the glass extends uninterruptedly over the entire lower surface and even over the dark spot. Thus he believes that he sees two colours together at this place, that is, green which he attributes to the glass plate, and pink-red which he attributes to the paper behind it and the two of them together do give the true colour of this place, that is white. As a matter of fact an object which seen through a glass sends white light to the eye as this spot does, would have to be pink-red. But when a white object of exactly the same appearance is placed above the plate of glass every reason for resolving the colour of the object into two disappears and it looks white to us.

This appears to assume that a colour passing through a coloured glass is added to the colour of the glass, whereas of course the result is incalculable beforehand and depends on the particular constitution of the glass. Moreover, as Hering has remarked, children see the complementary colour. Are they aware that rose and green are complementary, and do they indulge in these elaborate calculations? Supposing in Helmholtz's argument we substitute a blue glass for the green one. The

¹ *Op. cit.* p. 286.

yellow of the pattern is quite as vivid as the rose in the case of green, but far from thinking that yellow and blue make a white or grey, ninety per cent. of people even to-day think they make green, and yellow seen through most blue glasses does actually look green. To estimate the black parts of the pattern as yellow in the way Helmholtz suggests, the light sent to the eye from them should be green and not grey as, according to Helmholtz, it actually is.

Hering's interpretation is strongly borne out when the experiment is made a comparative one.

(i) *New form of the Scina experiment.*

To this end I used, instead of a single coloured glass for the whole field, two strongly contrasting glasses, one a full rich blue, and the other a fairly deep yellow. They divided the field centrally into a right and left half, so that one half was seen through the blue glass and one half through the yellow.

It was easy to verify by covering the whole floor with a black flock paper, that the light actually sent to the eye by the dark parts of the pattern was tinged by reflection by the colour of the overlying glass.

I used for a pattern in the first experiment, a double **V** like the pattern used by Hering to illustrate the contrast of black and white. The double **V**'s in the pattern on the floor were black on a white ground, and so arranged that their apices met on the central line just under the division between the glasses.

If the experiment was then made with perfect strictness, the difference between the **V**'s was manifest, the one under the yellow looking bluish, and that under the blue, yellow. All inexperienced people saw this difference, though some might call one arm grey and the other coloured.

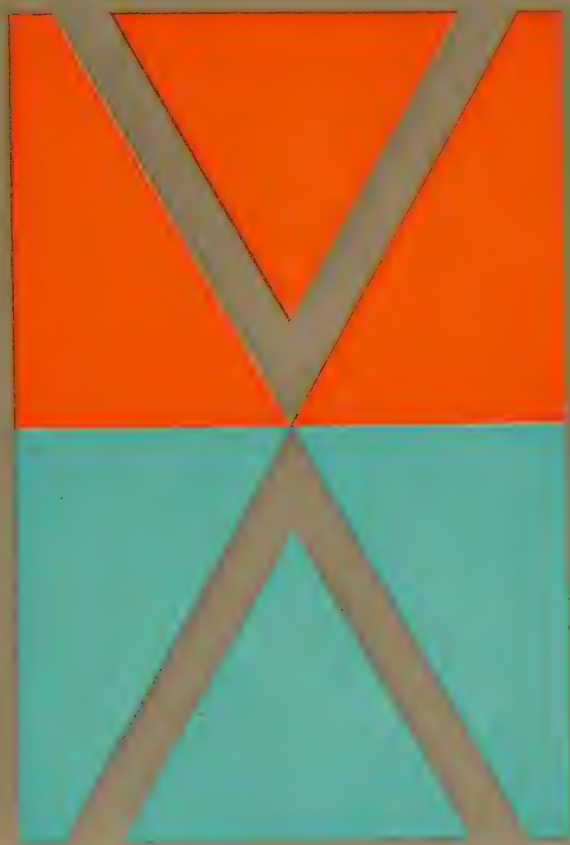
It is a fact, and one that goes far to refute Helmholtz's explanation of the Ragona Scina experiment, that the contrast between the two **V**'s can be just as easily seen without the elaborate arrangements of the experiment.

Over one half of a rectangular grey paper I pasted a saturated blue paper, and over the other a saturated yellow paper, leaving a double **V** of the same grey as the ground to meet at the apices on the line separating the blue and yellow halves. Using similar care to avoid successive contrast, the two greys were just as sharply contrasted as in the former experiment, and observers were astonished when they were told that they were both exactly the same grey colour, apart from their surround-

ings. I think it is very significant that when a comparative method is used, the artifice of the Ragona Scina experiment has no advantage in the display of contrast over the most straightforward proceeding. Recurring to that experiment, I tried the **V**'s in various positions, also the target type of pattern used by Hering, but when proper care to avoid successive contrast was taken, the contrasting colour always appeared under the respective glasses. This was also the case even when a straight bar pattern was used, the contrast in this case being more marked than in the parallel experiment with grey paper. In the latter case, when a bar of suitable grey crosses the centre of a paper of which the right half is green or blue and the second half yellow or red, the contrast effect is absent or faint. There is a distinct tendency to see the bar of a uniform grey, and many people call it so. I myself always seem to see some difference between the two sides, and if a black strip separates the two sides the contrast becomes manifest. I think that the division between the two glasses in the Ragona Scina experiment plays the part of the black strip in the case of the coloured papers.

It appears, then, that for some people at any rate, the continuity of the grey strip suggesting a uniform colour prevents colour contrast being seen even in a comparative experiment. This result is even more marked if the grey strip is part of a larger sheet of which the comparative experiment occupies only a small portion. In this latter case not even the use of tissue paper seems to me to make the contrast colour visible. If, however, the central portion containing the comparative experiment is isolated from the rest by a narrow black strip and a black strip separates the colour, the contrasting colours on the grey bar are manifest.

There is an interesting variant of this experiment upon a general ground of grey paper which both emphasizes the power of suggestion in visual perception, and strengthens the case for simple induced effects in Hering's sense. If I place in the centre of a grey field double contrasting **V**'s occupying rather a small proportion of the whole field, say a sixth or an eighth, so that the grey portion of the double **V**'s forms a part of and is continuous with the field, and then fix the apex of the **V**'s, not only are the contrast colours of grey strips which converge to this apex plainly visible, but the contrasting colour extends to each half of the whole field. By fixating, not the apex of the **V**'s but a point in the grey field just above the vertical line bisecting the **V**'s, I can recover the impression of the uniformity of the field, but I cannot abolish the difference between the two strips which converge to the apex of the **V**'s.



TENNANT.—THE PSYCHOLOGICAL FACTOR IN COLOUR CONTRAST.

10. METHODS OF INCREASING SIMULTANEOUS CONTRAST.

Lastly I shall discuss the well known and sometimes rather surprising effects of tissue paper in increasing the perception of contrast.

Prof. Allen's explanation of this seems to me quite inadequate. He says¹: "The tissue paper reflects upon the active retinal areas a small amount of white light which contains all the colour of the spectrum. An additional stimulus for each enhanced sensation is thereby provided and the reciprocal modification of the colour is made more effective and complete." There are two pertinent criticisms to be made on this. In the first place, as may easily be verified, the tissue paper does not in all cases add white. A pure white disc is darkened by it, the loss by absorption being greater than the light reflected from the surface. If the white content is increased, as is the case with nearly all greys, the increase in white value can be imitated by using a lighter grey on the surface of the tissue paper, or by using a lighter grey without using the tissue paper at all.

In the second place, no account is taken in this explanation of the much diminished saturation of the colour of the inducing field, and the consequent weakening, according to the measurements of Hering and others, of the induced effects². Indeed, with such tissue paper as I have used, the inducing field is sometimes so faint and irregular that some people see the contrast colour on a dark disc placed below it almost more clearly than the colour of the ground itself. In some of these experiments the very differences of different observers seem to suggest a factor other than a purely physiological one.

I find that a thin glass carefully ground may be used to replace fine transparent tissue paper with somewhat similar results, but in this case the greater transparency of the glass diminishes much less the saturation of the ground colour, and makes much less change in the apparent white content of any grey placed below it.

I covered a dark but richly coloured and saturated royal blue paper, with tissue paper of exactly the same size and above that a finely ground thin glass of like size and concealed the edges by a black frame. The blue showed so faintly that some failed to recognize it, but a black flock paper placed below not only made the blue manifest, but showed to most people a yellow tint quite as definite as the blue itself.

¹ *J.O.S.A.* ix, 383.

² See Parsons' *Colour Vision*, 1st ed. pp. 127, 128, 264.

If I placed a light grey on the surface I could see no contrast colour when, as in this case, the ground colour was so faint.

If, instead of royal blue, I used a bright saturated green, the ground colour was more manifest, and to me a grey placed on the surface showed the complementary purple, though not to all observers, and a black disc placed below the tissue not only accented the manifestation of the green, but the contrasting purple was visible to everyone.

If I placed a grey disc above so that it lay partly on the ground glass and partly on the black frame, no colour was visible on the grey. If, instead of using both tissue paper and the ground glass, I used the ground glass only, the contrast colour was manifest to me whether I placed a grey disc below or on the top of the glass, and it was possible to so choose the upper grey as to make a fair match with a very slightly darker grey placed below, thus repeating the experiment which both Helmholtz and Hering made with the Ragona Scina experiment with such diverse conclusions. In this case I always see the contrast colour on both greys.

It is remarkable in tissue paper experiments how very faintly perceptible the ground colour may be and yet give effective contrast on a black or dark grey patch placed below the paper.

I believe the main factor in these experiments with faint colours is psychological, and depends almost entirely on the delicacy of our discrimination of faint distinctions of hue, and the comparative vagueness of our perception of absolute colour, especially in the pale tints approaching pure grey. I have already alluded to the illustration of this factor in experiments on complementary colour, while the colour-top and tissue paper experiments give interesting illustrations of it, apart from the production of contrast effects.

Thus, if I cover the left half of a rectangle with blue and the right half with red, I can so cover each half with tissue paper as to make its colour quite unrecognizable when seen alone with the other half concealed, and yet easily identifiable when both halves are visible at once.

It is experiences with simultaneous contrast of this type which partly justify Helmholtz's remark that contrast is most manifest with faint colours, and yet I do not think these appearances are inconsistent with the conclusions of Hering and others that in quantitative experiments the greater the saturation of the inducing field, the greater is the measured amount of contrast.

In experiments with the use of ground glass only, or equally transparent tissue paper, I think the advantage in contrast effect of the tissue paper is to be explained on that principle of the recognition of normal

colour which I have argued explains the apparent advantage for the same purpose of the Ragona Scina apparatus. When a comparison between a grey above and a grey below the ground glass is made, advantage is seen to be only apparent, and whether the result of the comparison is thought to confirm or disprove the physiological explanation seems to depend largely on the personal equation of the observer. There is, I believe, a real conflict in such cases between the perception of contrast and the action of the principle of the recognition of normal colour, which leads different observers to different results.

I may add that when the double V's are used above and below the tissue paper, no differences are found by *any* observer in the result of the experiment.

11. SUMMARY.

To sum up the points we have been considering:

A. As to the reality of the physiological factor in simultaneous colour contrast:

(1) No *direct* light on this factor is thrown by experiments involving long fixation, as simultaneous colour contrast is an instantaneous effect diminished by long fixation.

(2) That the physiological factor is really operative is proved by comparative experiments as used by Hering for the case of light and dark, and by the use of the double V's and the electric torch experiment in this paper.

(3) Rollet's theory, explained and illustrated by McDougall, gives a reasonable explanation of the phenomena.

(4) Allen's extension of the induced effects to the senses of hearing and touch shows that in cases of fatigue they are a general property of nerve tissue and removes any *a priori* objection to their extension to instantaneous cases, whatever theory of vision we adopt.

B. The psychological factors:

(1) The principle of the recognition of normal colour, called by Hering "die Konstanz der Sehdinge." It operates in normal cases to conceal the induced effects, and the classical experiments are various devices to avoid its operation. It would be difficult to explain its action in normal cases if the induced effects were a direct action on the retina, as Green assumes was Hering's opinion, but if both McDougall and Allen are right in holding that they depend upon the central cortex, their disappearance in many cases is comprehensible.

(2) The principle of the relativity of our perception of visual effects, and our uncertainty as to absolute hue. This is specially stressed by Helmholtz and by Green. It is chiefly operative with faint and delicate tints and appears to be the principal factor in the tissue paper experiment.

(3) The action of suggestion. This is fully discussed by von Kries, and is not the special subject of this paper, but it is probably one of the factors operating in the coloured lights experiment.

(Manuscript received 20 June, 1928.)

QUANTITATIVE MENTAL ESTIMATES OF LIGHT AND COLOUR

BY LEWIS F. RICHARDSON.

1. *Introduction* (p. 27).
2. *Numbering browns* (p. 28).
3. *Locating pink on a mental scale of redness* (pp. 29–36).
 - (1) *Preparations and methods* (pp. 29–32).
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6. *Summary* (p. 37).

1. INTRODUCTION.

PSYCHOLOGY will never be an exact science unless psychic intensities can be measured. Some authorities say that such measurement is impossible*. Many estimates of the intensity of auditory imagery have been made by the present author†, but some psychologists were incredulous. A research was therefore pursued, in collaboration with J. S. Rose, M.A., on Loudness and Telephone current‡, and is found to show that the estimates were significant, the mean uncertainty of an individual estimate being the ratio 1 : 1.6. In the present paper the enquiry is extended to visual sensations, the question being: How do people agree or differ in their estimates of visual sensations? A further paper by R. S. Maxwell, M.A., deals with the functional relation between stimulus and red sensation. Guesses at mid-tints have been studied statistically by Karl Pearson§ some thirty years ago.

* W. James, *Principles*, 1901, I, 546; also C. S. Myers, *Textbook of Experimental Psychology*, 1925, I, 252. But contrast Brown and Thomson, *Mental Measurement*, Ch. I.

† L. F. Richardson, "Conation, Imagery and Cerebral Conductance," *Journal of General Psychology* (Clark University Press, U.S.A.), April 1929.

‡ L. F. Richardson and J. S. Ross, "Loudness and Telephone Current," accepted by aforesaid journal.

§ Karl Pearson, "Contributions to the Mathematical Theory of Evolution," *Phil. Trans. A*, CLXXXVI, 392.

2. NUMBERING BROWNS.

1927, Oct. 11. On a rectangle of brown paper, 54×39 cm., were mounted four paper squares each 14×14 cm. and severally white, buff, brown and black. The whole was set on a drawing-board and illuminated by gas-filled tungsten lamps, shining nearly normally on it. It was arranged to be viewed from a distance of about 140 cm., and rather from one side.

Thirty-six men, studying for the Intermediate B.Sc., at Westminster Training College, were given, during an examination in practical physics in which each student did in turn the same 36 tasks of four minutes each, the following written instruction:

"Stand in the chalk ring and look at the drawing-board. On a scale of lightness and darkness, call the black square 0, the white 100. What numbers do you then give to (a) the background, (b) the brown square, (c) the paler square?" The time allowed to each man was four minutes. They were kept in ignorance of each other's opinions till all had been recorded. As far as I could ascertain, no one of them had had any previous training or practice in the assignment of numbers to tones. No one complained that the task was impossible; but one man put down sizes by mistake.

The paper was then covered to protect it from dirt and light.

1927, Oct. 15. The same men being seated in a class-room, I said to them that about half the class had been of one opinion as to the order of the tones and about half of the other; and that I should like to have their considered judgment. The board was mounted in view of all and the room illuminated by gas-filled tungsten lamps, daylight being excluded. On the blackboard I wrote *o* black, *a* background, *b* brown square, *c* paler square. I also told them to look at the contrasts.

At this second attempt they all gave the same sequence except one man, and when questioned he immediately conformed.

The results are set out below.

		<i>a</i>	<i>b</i>	<i>c</i>
Oct. 11.	Means for the 21 students who maintained the same sequence on Oct. 15 ...	34.5	52.9	86.8
Oct. 15.	For all 36 students:			
	Means	29.3	50.4	83.8
	Least	15	30	60
	Greatest	55	70	96
	Standard deviation from mean	12	12	11
		<i>a, b</i>		<i>b, c</i>
	Correlation coefficients (product moment)	0.7		0.6
	Probable error of correlation	0.06		0.07

3. LOCATING PINK ON A MENTAL SCALE OF REDNESS.

(1) *Preparations and methods.*

This enquiry was provoked by the remark of William James: "To introspection, our feeling of pink is surely not a portion of our feeling of scarlet*."

The *white* used was that of a pure white cardboard sold for mounting pictures. The *scarlet* was Messrs Winsor and Newton's 'scarlet poster card.' It nearly matches the scarlet variety of mercuric iodide.

The pink was provided in different ways in the apparatuses known as 'Triangles' and 'Wheel.'

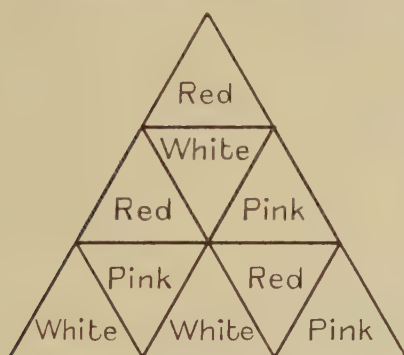


Fig. 1.

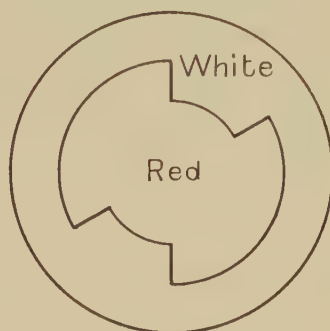


Fig. 2.

0 10 20 30 40
Scale of cm.

Triangles. Pale red drawing paper, sold by the Old Water Colour Society, was cut into equilateral triangles 13 cm. in the side, and was mounted with equal scarlet triangles on the white card in the pattern shown in Fig. 1. This pattern has three advantages: (i) the white, pink and scarlet occupy equal areas; (ii) each is in contact with the other two; (iii) such contacts are triplicated so as to help the observer to eliminate any slight variations of illumination across the pattern.

In a separate experiment the pink paper was compared with mixtures of the white and the scarlet on a colour wheel. It matched for darkness when the angle of red was 210, 217, 210, 245†, 245† by electric light, 205 by daylight, the rest of the 360° being white. When thus matched

* W. James, *Principles of Psychology*, 1901, I, 546,†;

† 245 by a poster artist, Mr Clifford Gabriel; other estimates by author. We agreed about saturation and yellowness.

30 Quantitative Mental Estimates of Light and Colour

for darkness the pink paper matched also for saturation but was decidedly too yellow.

(Note added 1928, Dec. 21.) The reflectivities of the white, pink and red corners of the set of triangles shown in Fig. 1 have now been measured on a photometer for six ranges of wave-length. As a standard white, a slab of plaster of Paris was prepared. It was cast on plate-glass from powder sold by the British Drug Houses, Ltd.; the dark spots were cut out and refilled; and the polish, visible at nearly grazing incidence, was removed by fine emery cloth. The plaster was dried before use. This white surface, *A* in Fig. 3, was set up at one end of a photometer-bench and was illuminated by light diverging from *C*, a hole 0.2 cm. diameter, on which light from a white hot tungsten ball was concentrated by lenses. The cone of diverging light was so narrow that scarcely any of it fell

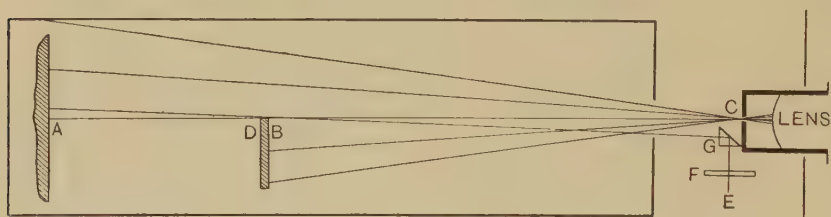


Fig. 3. Diagram of Photometer.

on the black materials which enclosed the apparatus. The triangle for test was placed at *B* on a stiff mount. The back *D* of the mount was covered with black paper; so were also the five triangles not in use. The triangle and the plaster were viewed by an eye *E*; through a colour filter *F*, from near the hole *C*. To get the viewpoint near enough to *C* it was necessary to use the reflecting prism *G*, when *B* was near *C*. The distance *AB* was varied until *B* matched *A*. The reflectivity of *B* relative to that of *A* was then assumed to be $(BC/AC)^2$ in which *BC* and *AC* are distances. The range of wave-lengths transmitted by the filter was measured separately by a spectrometer.

The results were as follows:

Colour	Range of wave-lengths in microns	Reflectivities if plaster has unit reflectivity			
		White mounting board <i>x</i>	Pink paper O.W.S. <i>y</i>	Scarlet poster card W. & N. <i>z</i>	$\frac{210z + 150x}{360}$
Blue	0.42 to 0.48	0.81	0.48	0.11	0.40
Green	0.47 to 0.54	0.83	0.46	0.08	0.39
Green	0.51 to 0.58	0.84	0.50	0.09	0.40
Orange	0.59 to 0.60	0.86	0.59	0.32	0.55
Red	0.61 to 0.67	0.87	0.79	0.72	0.78
Red	0.65 ₅ to 0.69 ₅	0.91	0.77	0.76	0.82

The uncertainty of the measured reflectivity is about ± 0.02 . In connection with the colour-wheel estimates, the function $(210z + 150x)/360$ is given in the last column; it should be compared with y . The excess of orange and green in y -is obvious.

Wheel. The pink is here made by rapid alternations of the scarlet and the white on a disc driven by an electric motor at a speed sufficient to prevent any flicker. The white and scarlet were nominally the same as those used for the triangles. Fig. 2 shows the disc. In the middle zone, the angle occupied by scarlet was $240^\circ \pm 0.5^\circ$; that is $\frac{2}{3}$ of the whole. The advantages will be discussed later.

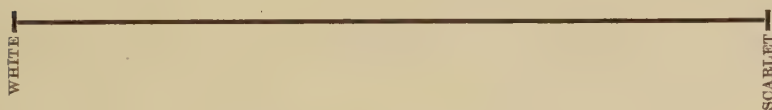
Procedure and instructions. Those about to observe the wheel were carefully kept in ignorance of the angles of red and white, and were allowed to see the wheel when it was rotating only.

Whereas for the browns, the observers were asked to assign numerals, for the pinks they were asked to divide a segment of a line 100 mm. long. This plan was suggested to me by Mr Ralph D. Richardson to make judgment easier, and I believe it does so. The typewritten instructions were as follows:

Comparing colour intervals.

Please look at the white, pink and scarlet (triangles). In degree of redness is the pink nearer to the white or to the scarlet?

Below is a line marked 'white' at one end and 'scarlet' at the other. Please put a mark on this line to show where you feel that the pink comes on this scale of redness.



Sign here

Date

Daylight }
Electric } cross out one.

If the observer showed difficulty in understanding the typed instructions, a verbal explanation was sometimes added to the following effect: "If the pink looks almost the same as the white, put a mark here (pointing to end of line labelled white). If the pink looks almost the same as the red, put a mark here (pointing to end of line labelled scarlet). If the pink is equally like the red and the white, put a mark in the middle of the line. But it is your own opinion that I want. As far as I know, there is no 'right answer.' Everyone's opinion is equally good."

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If the observer put the mark very far away from the rest of the group I sometimes discussed the matter further and was surprised to find that these extreme estimates were generally not due to any misunderstanding of instructions. One paper was self-contradictory and was rejected.

Conversion to numbers. Throughout the scale is

White = 0 Scarlet = 100

The 'estimate' for the pale red is defined to be the percentage of the length of the line from the end marked white up to the mark. The lengths were measured by me, using a scale engraved on glass. Throughout the arithmetic, numbers are rounded off to the nearest unit percentage, or if exactly midway, then to neighbouring integer which is even.

(2) Tabulated estimates.

Table I. *Triangles.*

		Men									Women				
Group reference	...	1	2	3	4	5	6	7	8	9	10	11	12	13	
Daylight or electric	...	<i>d</i>	<i>d</i>	<i>e</i>	<i>d</i>	<i>e</i>	<i>d</i>	<i>d, e</i>	<i>e</i>	<i>d, e</i>	<i>e</i>	<i>d</i>	<i>e</i>	<i>d</i>	
No. who tried and failed	...	0	0	0	1	0	0	0	2	0	1	1	0	0	
No. who made estimates		33	15	25	8	9	8	4	9	11	25	10	6	6	
Estimates	{Greatest	...	46	40	76	59	67	33	21	51	39	60	76	52	35
	{Median	...	36	33	37	34	34	26	18	29	32	36	26	34	28
	{Mean	...	36	30	40	34	37	26	17	30	30	37	36	36	26
	{Least	...	20	12	25	19	13	18	12	8	17	14	20	25	14
Standard deviation of individual from group-mean			6	7	11	11	17	5	3	10	6	11	20	9	7

Group index:

- | | | | |
|-------|---|---|---|
| Men | { | 1. Class for Intermediate B.Sc. | } Westminster Training College, London. |
| | | 2. Class for Diploma in Education | |
| | | 3. Class for drawing (Board of Education) | |
| | | 4. Teaching Staff. | |
| | { | 5. Quakers. Social gathering. London, N.W. Ages 20 to 70. | |
| | | 6. Art students. Roy. Coll. Art. Teachers' Course, Post Diploma. | |
| | | 7. Other teachers and inspector of drawing and painting. | |
| | | 8. Fellows of the Royal Society. | |
| | | 9. Unclassified; mostly university graduates. | |
| Women | { | 10. Quakers, the same as 5, but women. | |
| | | 11. Art students, the same as 6, but women. | |
| | | 12. Unclassified, mostly relatives of group 9. | |
| | | 13. Class in Advanced Psychology Univ. Coll. London. Mixed sexes. | |

Supplementary Index for Wheel:

- | | |
|-------|--|
| Men | 14. Class for Intermediate B.A., Westminster Training Coll., London. |
| Women | 15. Wesleyan Missionary Society's Garden Party, London. |
| Men | 16. Unclassified men. |

Table II. *Wheel.*

				Men								Women	
Group reference	...	...	...	1	14	2	4	6	7	8	16	11	15
No. who tried and failed	...	...	...	0	0	0	0	0	0	1	0	1	0
No. who made estimates	...	...	...	29	16	18	6	8	2	12	20	10	36
Estimates	Greatest	...	...	58	62	83	63	74	59	78	65	89	86
	Median	...	...	40	40	40	48	52	.	35	42	66	44
	Mean	...	...	39	41	46	46	54	.	44	43	59	47
	Least	...	...	26	29	29	24	37	37	24	27	24	15
Standard deviation of individual from mean of group				...	...	...	...	...	...	...	...	...	...
				7	9	15	14	11	.	15	10	20	16

Note. The same individual is counted once only for the triangles and once for the wheel, even if, logically, he belongs in several groups.

For those who are interested in frequency-distributions the following summary is collected in ranges of 5 units.

Table III. *Frequencies.*

Centre of range of estimate	Triangles excluding group 13		Wheel	
	Men	Women	Men	Women
5	0	0	0	0
10	3	0	0	0
15	2	2	0	1
20	11	4	0	0
25	13	5	4	2
30	24	4	17	3
35	34	10	19	9
40	21	7	29	4
45	6	1	11	4
50	2	5	5	6
55	1	0	8	1
60	2	1	9	1
65	2	0	4	5
70	0	0	2	5
75	1	2	1	3
80	0	0	1	0
85	0	0	1	1
90	0	0	0	1
95	0	0	0	0
Total persons	122	41	111	46
Mean estimate	34	36	43	50
Standard deviation	10	13	12	17

(3) *Causes of difficulty and of variation.*

Observers have mentioned many causes. Although I have not thoroughly explored any variations, the following notes may be useful to future investigators.

(1) *Obliquity of view.* Care was taken to choose papers equally matt, so that the pink seemed to me in about the same relation to white and scarlet from whatever angle the card was viewed.

(2) *The change from daylight to electric,* might be expected to affect

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the triangles. The means are: daylight 36, 30, 34, 26, 36, mean 32·5; electric 40, 37, 30, 37, 36, mean 36, and the difference is scarcely significant.

With the colour wheel any such effect is probably less.

(3) *Confusion between darkness and saturation.* Several subjects were perplexed about this for the triangles. Some observers stated that they estimated by darkness, but I have not rejected their estimates on that account. With the wheel the estimate would probably be much the same whether judged by darkness or saturation.

(4) *Pink not of same hue as scarlet.* The *Concise Oxford Dictionary* describes pink as a "pale red slightly inclining to purple," and scarlet as a "brilliant red colour inclining to orange." Thinking that these contrary inclinations might perhaps have been all or part of the difficulty that James felt, I tried, in choosing the pale triangles, to make them of the same hue as the red. But evidently I overshot the mark, for many subjects objected, stating that the 'pink' was apricot, buff, brownish, flesh coloured, terracotta, too yellow, too opaque, too grey. Indeed two observers in Group 8, who said that the task was impossible, gave this difference of hue as their reason.

The colour wheel was made to avoid this difficulty and at the same time to give much the same result whether the observer judged by saturation, as the instructions indicate, or by darkness. Yet even when the pink was made by mixing the white with the scarlet on a colour wheel an Art Student remarked "pink is slightly purple—probably slightly off the true scale" and six others in the same group made similar if less explicit comment. One other subject said that the pink was too blue.

These experiences suggest that people vary a good deal in their sensitivity to light waves of lengths between 0·6 and 0·7 micron.*

(5) *Half closing the eyes.* Two observers stated that this moved the pink nearer to the white.

(6) *Distance.* Two skilled observers remarked that the estimate increased with distance, roughly thus:

		1 m.	2 m.	6 m.
Triangles, electric light	...	15	—	21
Wheel		25	36	49

But on sorting the estimates of Group 1 on triangles any effect of distance was lost in the general scatter. The distances used were between 1 and 5 metres.

* As is well known. See Sir J. Herbert Parsons, *An Introduction to the Study of Colour Vision* (Cambridge University Press), 2nd ed. p. 193.

(7) *Bias by pigments.* One observer, who gave the lowest estimate in Group 5, remarked that he was a dyer and was influenced by a knowledge of the strength of dye required to make the paper. Accordingly I asked the Art students to put from them all thought of quantities of pigment and to judge the colours simply mentally. But, having tried, about half of them protested that they could not thus put aside their experience.

(8) *Practice.* Groups 6 and 7 have of course had most practice, but also most opportunity for bias by pigments. No one remarked on having tried this sort of task before; and I provided no practice beyond the repetitions here recorded. That is to say, the group that made most repetitions was No. 1, the Intermediate Science class, and their successive standard deviations were:

1927 Oct. 11	1927 Oct. 15	1928 June 11	1928 July 12
Browns	Browns	Pink wheel	Pink triangles
Confused	$\begin{cases} 12 \\ 12 \\ 11 \end{cases}$	$\begin{matrix} - \\ 6.6 \\ - \end{matrix}$	$\begin{matrix} - \\ 6.2 \\ - \end{matrix}$

Much practice would be a desirable preliminary to future researches.

(9) *Quick changes.* One observer of the triangles marked his first impression as 8, changing during about three minutes to 26.

Another estimated the triangles at 29 by first impression and added that "reasoning would tend to bring the mark half-way—if not nearer to scarlet."

Another said of the wheel that the pink "appeared to become paler the longer the colours were viewed."

Another agreed with this last statement.

(10) *Abnormalities of colour vision.* That eyes may differ is shown by an educationalist, aged 77, who viewed the triangles by electric light and reported that by right eye he saw scarlet and gamboge; estimate 27. By left eye, which is partly blind to red, he saw brick red and lemon yellow; estimate 13. Other estimates by men who said that their vision was peculiar were:

Abnormality	Triangles		Wheel
	daylight	electric	
Unknown type ...	—	26	—
Unknown type ...	38	—	31
Blind for red ...	—	34	—
Weak for red ...	—	—	24
Weak for green ...	—	—	30

(11) *Sex.* The means for men and women are shown separately at the foot of Table III. On the wheel women see the pink redder than do the

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men and the difference is perhaps significant, for the means are 43 ± 0.8 , 50 ± 1.7 where the numbers following the $\pm$ sign are 'probable errors.'

(12) *Age*. The observers mentioned in Tables I, II, III were all over 17. A few children have also looked at the triangles and at two other wheels. Below 12 years, estimates were sometimes very high, as if erratic.

(13) *Persistence of individual differences*. This was sought for by plotting correlation diagrams in which the co-ordinates of a point are the estimates of the Triangles and of the Wheel made by the same person. This was done for Groups 1, 2, 6, 11. But there was no significant correlation. We might expect to find persistent individual differences if the observers had much more practice. For the browns there was the significant correlation stated in § 2.

(14) *Reasoning*. Mr Dudley Heath made the interesting suggestion that those art students who decline to make a decision in this and similar problems are often those who are also argumentative; and that they could place the mark on the line if they would trust to their first impressions, instead of to reasoning.

4. REPLY TO AN OBJECTION.

The quotation from James continues "...nor does the light of an electric arc seem to contain that of a tallow candle in itself." Even if we could abstract brightness as a quality of the light, it would be difficult to compare the brightnesses, because the arc is dazzling.

Similarly it is difficult to compare the loudness of a pistol-crack with that of a cannon-roar¹ because the roar is deafening.

Here is another specimen. It is very difficult to say how many times less painful it is to be pricked by a pin than to have a tooth extracted without anaesthetic; because the dental pain is overwhelming.

But these are no arguments against the estimation of less violent sensations.

James in this passage was perhaps more concerned to emphasize the words *portion* and *contain*. That is another question. There is no evidence that any of the present observers counted up little bits of pinkness, and no objection to James's remarks about addition.

5. ACKNOWLEDGEMENTS.

My best thanks are due to the many observers for their careful opinions; and also especially to those who helped to organize the work,

¹ C. S. Myers, *Textbook of Experimental Psychology* (1925), Part I, 252.

namely Dr H. H. Dale, Sec. R.S., Prof. Flügel and Mr Raper of University College London, Mr Wellington and Mr Dudley Heath of the Royal College of Art, and to Principal Workman, Mr Patten and Mr J. S. Ross of Westminster Training College.

The arithmetic was performed on a machine lent by the Government Grant Committee of the Royal Society.

6. SUMMARY.

(1) Of 164 persons who were asked to estimate quantitatively the redness of pink paper, only five said that the task was impossible. With a pink made on a colour wheel only two out of 159 found the task impossible.

(2) The pink, made by mixing $\frac{2}{3}$ of scarlet with $\frac{1}{3}$ of white on a colour-wheel, was judged mentally to be at the following positions on a scale in which 0 is white, 100 is scarlet. Mean estimate by men 43, by women 50. There is thus a remarkable disagreement between angles and mental estimates. Mr R. S. Maxwell, M.A., is exploring this further.

(3) The standard deviation of an individual estimate from the mean for the same sex is about 13 of these units. Many possible causes of this variability have been discussed.

(Manuscript received 16 August, 1928.)

A FURTHER EXPERIMENT ON AUDITORY FATIGUE

BY FRANK A. PATTIE, JR.

(*From the Psychological Laboratory, Harvard University.*)

1. *Discussion of earlier work* (pp. 38-39).
2. *New experiment described* (pp. 39-41).
3. *Results* (p. 41).
4. *Conclusions* (pp. 41-42).

1. DISCUSSION OF EARLIER WORK.

IN 1920 J. C. Flügel¹ reported experiments on the shift of localization of a binaural tone which occurs after 'fatigue' of one ear by stimulation with a continuous tone. The apparent localization is shifted towards the unfatigued side. In this paper this effect will be called 'localization-fatigue.' In 1927 the writer² reported experiments, following investigations by Flügel and others, on the diminution of intensity of tones after prolonged stimulation. This effect will be referred to as 'intensity-fatigue.'

In 1922 F. C. Bartlett and H. Mark³ performed an experiment in which they obtained the same shift of localization as in Flügel's work, but after a period of binaural stimulation in which the tone conducted to one of the ears was leading in phase with respect to the other tone. It seemed to the subjects that they were being stimulated in only one ear, that in which the phase was leading, and the 'localization-fatigue' occurred just as it would have occurred if they had been stimulated in that ear only. These investigators attribute the effect not to local fatigue but to an "error of judgment." "The extreme right or left position having been presented for some time, a 'set' is established as a result of which a marginal position appears more central than it normally does." Flügel would perhaps not be unprepared for such an experimental result, for he distinguishes "fatigue of the process of binaural localiza-

¹ "On local fatigue in the auditory system," this *Journal*, XI, 105-134.

² "An experimental study of fatigue in the auditory mechanism," *Amer. J. Psychol.* XXXVIII, 39-58.

³ "A note on local fatigue in the auditory system," this *Journal*, XIII, 215-218.

tion" and the fatigue which causes diminution of intensity, and his data for localization-fatigue vary so widely among themselves as to suggest the participation of psychological factors in the production of this effect.

The experiment of Bartlett and Mark did not rule out the possibility of a differential intensity-fatigue, which might have been produced in the two ears because of the difference in phase of the two tones. The results of the work of von Hornbostel and Wertheimer¹ and of Klemm² (which showed that if two clicks, qualitatively alike but separated by a very short interval of time, are presented separately to the two ears only one click is heard and its localization is on the side of the earlier) and the facts of the effects of binaural phase differences on localization of tones led the writer to expect that a different degree of intensity-fatigue would be shown if the ears were stimulated as in Bartlett and Mark's investigation and the intensities of a tone as perceived in each of the ears were afterwards compared. The experiments on the localization of clicks and those on binaural phase relationships as determinants of localization may be considered as two cases of one phenomenon, namely, the inhibition of a later excitation by an earlier, if we assume that there are certain maxima of excitation produced in the auditory nervous mechanism which correspond to certain periodic features of the tonal vibration. If the series of later excitations is centrally inhibited by the earlier, it seems reasonable to expect that the physiological effects of the later excitations should be abolished. The effect of continued stimulation is a diminution of tonal intensity, and it is conceivable that this effect might vanish in that ear which is stimulated by the tone which lags in phase behind a tone presented to the other ear. This hypothesis assumes that the seat of intensity-fatigue is central rather than peripheral, but at present we are in ignorance as to the actual state of affairs; Bleyl³ suspected that it was central, for he found that his neurasthenic subjects were unusually fatigable when he gave them tests with tuning forks, and he assumed that the general fatigability associated with neurasthenia was central in origin.

2. NEW EXPERIMENT DESCRIBED.

In the experiment designed to test this hypothesis on the relationship between binaural phase differences and intensity-fatigue, two kinds of stimulation were employed alternately in every experimental sitting.

¹ *Sitzungsber. d. preuss. Akad. d. Wissensch.* 1920, 388-396.

² *Arch. f. d. ges. Psychol.* 1919, xxxviii, 105-110; 1920, xl, 117-146.

³ *Arch. f. Ohren-, Nasen-, u. Kehlkopfheilk.* 1921, cviii, 192-197.

The subject was first stimulated binaurally with two tones which were out of phase to such an extent as to make the localization appear wholly on the side of the tone with leading phase. After this type of stimulation, in which the subject thought he was being stimulated uniaurally, the condition of the ears as to intensity-fatigue was tested. If intensity-fatigue should be found in the ear on the side of the leading phase and not in the other ear, the hypothesis would be verified. The subject was then stimulated uniaurally on the side on which the phase had led in the period of binaural stimulation, and the degree of intensity-fatigue of the stimulated ear was then measured; this second kind of stimulation was used in order to make sure that the binaural tones were strong enough to produce intensity-fatigue when acting singly. These two types of stimulation were called *A* and *B*, respectively; a more detailed description of them follows:

A. Binaural diphasic stimulation for 1 minute with a loud tone of 512 volts produced by a Western Electric 2-A audiometer, amplified by means of a Western Electric 25-A amplifier, and conducted from a telephone receiver in a sound-proof box through a Y-tube, the branches of which were connected to rubber tubes leading to the subject's ears. The loudness of the tone at the receiver was 130 'sensation units.' The length of tubing from the source to the ear was about 6.3 m. The diameter of the tubing was 1.5 cm. The tube leading to one ear was lengthened so as to throw the tone in that ear approximately half a wave-length behind the other tone. Localization of the binaural tone was entirely on the side of the leading phase. This change in phase did not cause any appreciable difference in the intensities of the two tones.

B. Uniaural stimulation for 1 minute from the same source and with the same conduction as in *A*, but with the connection broken between the branch of the Y-tube and the rubber tube which in *A* conducted the tone of lagging phase. The branch of the Y-tube was stuffed with cotton wool; in a later technique it was connected to a rubber tube, plugged at its end with cotton wool, which was as long as the tube leading to the ear. This method was used in order to make the uniaural stimulus strictly identical with the stimulus on the same side which was acting when *A* stimulation was employed.

A and *B* types of stimulation were employed alternately until each had been used ten times in the experimental sitting.

The test for intensity-fatigue was made according to the writer's method described in the paper already cited. Two tones, very near subjective equality when the ears are normal, were successively presented,

one in each ear, for comparison after the minute-long stimulation. Intensity-fatigue in one ear will be shown by an increase in the number of judgments of 'less' and a decrease in the number of judgments of 'greater' given with respect to the tone presented in the 'fatigued' ear.

3. RESULTS.

The results of the experiment showed that binaural diphasic stimulation does not produce any differential intensity-fatigue; both ears are equally fatigued by such stimulation. The hypothesis is therefore not verified, yet the data are nevertheless valuable, for certain conclusions may be drawn from them. The three subjects used gave the same results. A typical set of data obtained from two hours' work with one of the subjects follows:

1. *A* stimulation with phase leading in the left ear. The two comparison-stimuli used to test for intensity-fatigue were presented in the order left-right, and the subject reported whether the loudness of the second tone was greater than, equal to, or less than that of the first; hence, if the left ear had been fatigued to a greater extent than the right, there would be an unusually large proportion of 'greater' judgments soon after the stimulation. The results are:

Number of judgments out of a total of 20 at						
	5	10	15	25	35	45 seconds after <i>A</i> stimulation
Greater	2	4	3	4	3	7
Equal	8	8	7	8	8	8
Less	10	8	10	8	9	5

2. *B* stimulation of the left ear for 1 minute. Same presentation of comparison-stimuli. Fatigue of the left ear is shown by the distribution of 'greater' and 'less' judgments and the decrease in the number of the former, accompanied by an increase of the number of the latter, with passage of time.

Number of judgments out of a total of 20 at						
	5	10	15	25	35	45 seconds after <i>B</i> stimulation
Greater	15	13	12	7	5	1
Equal	2	4	3	4	2	9
Less	1	3	5	9	13	10

4. CONCLUSIONS.

The conclusions that may be drawn from the experimental data are these:

1. Bartlett and Mark's experiment is freed from the objection that they had not ruled out the possibility that their method of stimulation might have produced a differential intensity-fatigue such as was looked for in the present investigation. Although we knew that it requires a considerable binaural difference of intensity to cause a shift of localization from the median position, we did not know that a binaural intensity-difference might not be at least a co-operating factor in their experimental situation where localization-fatigue was produced.

2. We have some ground for believing that the seat of intensity-fatigue is peripheral and not central. The argument, which is *formally* valid, is: If intensity-fatigue is central rather than peripheral in origin, differences of phase will produce a differential fatigue-effect in the two ears: but such an effect was not produced; therefore, the fatigue is not central.

3. Subjective factors have nothing to do with intensity-fatigue. In 1908 Sewall¹ performed experiments which led him to think that intensity-fatigue was entirely conditioned by attention. In the writer's former study of it, he attempted to demonstrate the irrelevancy of subjective factors by the greatest possible variation of the experimental method, but he was not able to *prove* it. In this experiment the subjects thought they were being stimulated in only one ear; therefore, the subjective conditions of the former experiments with uniaural stimulation were exactly realized, yet fatigue was produced both on the side on which the stimulus was perceived and also to an equal extent on the side on which no stimulus was perceived. (There is an inescapable consideration which must be mentioned here: it is merely assumed, because of the principle of continuity, that both ears were *fatigued* by the binaural stimulation. As a matter of fact, the results show only that both ears were in the same condition with respect to sensitivity. We have no way to determine whether they were fatigued or not, since we have no third ear which could be kept isolated from stimulation and used as a standard for the measurement of fatigue in the other two. There is no reason, however, for doubting that they were in a fatigued rather than in an unfatigued condition.)

¹ *Zeitschr. f. Sinnesphysiol.* 1908, XLII, 115-123.

(*Manuscript received 28 July, 1928.*)

A FURTHER STUDY OF THE PSYCHOLOGICAL EFFECTS OF LONG SPELLS OF REPETITIVE WORK

By K. G. POLLOCK.

(*From the Cambridge Psychological Laboratory.*)

- I. *Introduction* (p. 43).
- II. *Experimental work of Professor B. Muscio, Dr G. Teruoka and Miss Vernon* (p. 44).
- III. *Description of Guidit Experiment* (pp. 44-46).
- IV. *Results* (pp. 46-57).
 - (a) *Accuracy and speed* (pp. 46-48).
 - (b) *The effects of 'pauses'* (pp. 48-50).
 - (c) *The effects of feelings of tiredness* (pp. 50-51).
 - (d) *The effects of 'conscious attitudes'* (pp. 51-56).
 - (1) *Cheerfulness and confidence* (p. 51).
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 - (e) *The effects of 'distractions'* (pp. 56-57).
- V. *Conclusions* (pp. 57-58).

I. INTRODUCTION.

SEVERAL attempts have been made to investigate the relation between accuracy and speed of work, and duration of work. The results so far have been extremely variable, tending to show that the relationship depends on individual factors and the nature of the work, rather than on any general principle. This paper records a further attempt towards the elucidation of the problem and the results must be considered together with those of previous experiments.

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II. EXPERIMENTAL WORK OF PROF. B. MUSCIO, DR G. TERUOKA AND MISS VERNON.

Prof. B. Muscio¹, experimenting in 1922 with an Aiming Test and a Pendulum Test, concluded that inaccuracy in this type of work was a function of the rate rather than of the duration of work. Dr G. Teruoka², carrying out a further investigation with the Aiming Test, also found that increased speed and decreased accuracy were associated, but that after about two hours' work at any particular rate, there was a decrease in accuracy. Some further work on the Pendulum Test was done by Miss M. D. Vernon³; the results, although indefinite, were not in agreement with those of Muscio, as there was a gradual decrease in accuracy after the first hour of work. She then employed a Paper-folding Test and a Maze-tracing Test, and observed a decrease of output in the afternoons in both tests, and an increase of errors in the Maze-tracing Test. Thus in all of these experiments there is some evidence for decreasing quality of work with long spells, particularly with periods of about two hours' duration, but there is little or no agreement with regard to the causal factors in operation.

III. DESCRIPTION OF GUIDIT EXPERIMENT.

Since the Paper-folding and Maze-tracing Tests were troublesome to score, a new test was employed in the experiment described below. The apparatus was that of a game called 'Guidit⁴,' and the task consisted in guiding a small metal ball up an inclined plane with a fine knitting-needle, set in a thin handle. The plane was a wooden board, 13 inches

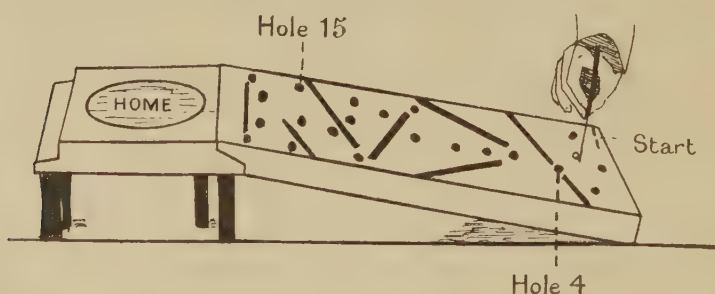


Fig. 1.

¹ *Report of the Industrial Fatigue Research Board*, No. 19 (General Series, No. 7), B.

² *This Journal*, XVI, 223.

³ *This Journal*, XVI, 222-236.

⁴ British manufacture. Maker unknown.

long by $3\frac{1}{2}$ inches wide, and inclined at an angle of about ten degrees to the horizontal. There were 21 holes in the board large enough for the ball to drop through and also seven barriers to be circumvented. The subject sat at a table, holding a stopwatch in his left hand, and after every attempt wrote down the place reached and the time taken. A system of marks was employed in order that some credit might be given when the ball, although not reaching the top, was manœuvred a certain distance. When the ball was taken to the top, 10 marks were scored and this was

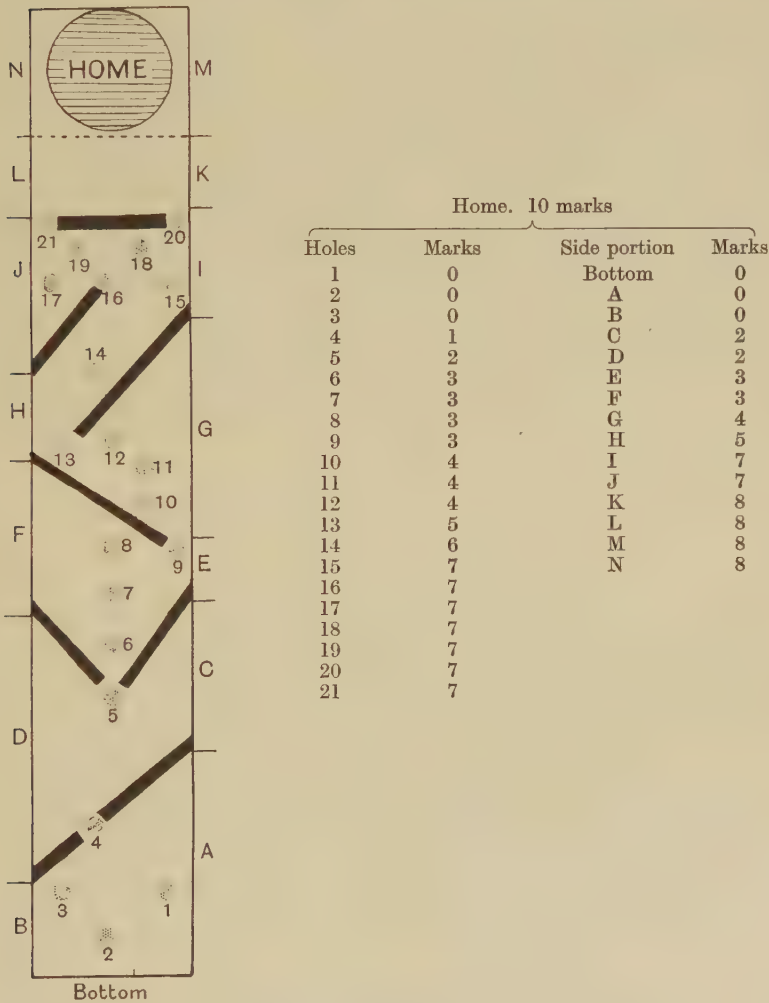


Fig. 2. Plan of Guidit board showing system of marking.

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called a 'success.' The rest of the scale was compiled after a few trials to determine as far as possible the relative difficulties at different points of the board. The scale is given in full on the preceding page (see Figs. 1 and 2).

Three subjects learned to perform the task, but one only performed the long spells, and this paper deals with her results alone. The subject started with practices of half an hour's duration and found that at first 40 attempts were made in this time. She then adopted a set of 40 attempts as a unit, and during the practice period worked sometimes one and sometimes two of these sets every day except Sundays and a few other holidays. Maximum accuracy was reached in the 29th set but was not sustained. After the 35th set, a vacation of six weeks intervened before the 36th, and the work immediately after the vacation was less accurate than before, accuracy improving up to the 47th set. After the 51st set she worked for six periods of about one hour, two sets of 40 attempts being worked in each period. They were performed in the mornings, four on consecutive days and the last two after an interval of one day. Six periods of two hours and six of four hours were then performed in the afternoons of three consecutive days in each of four weeks. On most of the intervening days there were short practice periods. Finally six eight-hour periods were worked on six consecutive days, on each of which there were two four-hour spells, with an interval of one hour for lunch.

IV. RESULTS.

(a) *Accuracy and speed.*

In order to determine whether there is any significant relation between the length of the working spell and the quality of the work done, average marks obtained for the whole spell and for various sections of the spell were computed. It is convenient to combine in a single table the average marks and numbers representing the average time taken for successful attempts. A 'successful' attempt is one in which the ball is taken 'Home,' that is to the top of the inclined plane. I am assuming that the average time taken for successful attempts fairly represents the general rate of work throughout the particular spell under consideration. This may, of course, be unjustifiable, and no doubt a more accurate record of the actual normal distribution of speed in movements throughout a 'successful' run ought to be made. No record is given of the actual number of attempts made during each hour, or other periods less than

the total, of work. Since records and introspections were taken down during the working periods, such numbers could have no important significance.

Table I.

Length of spells	Total no. of attempts		Mark whole periods	Time whole periods seconds	Mark 1st halves	Time 1st halves seconds	Mark 2nd halves	Time 2nd halves seconds
1 hr.	480	Average	8.7	19.5	8.8	20.5	8.7	18.5
		S.d.	2.2	3.7	2.0	2.2	2.2	3.4
2 hrs.	1440	Average	8.2	15.4	8.4	16.5	8.0	14.2
		S.d.	2.3	3.8	2.5	2.0	2.1	3.6
4 „	3700	Average	8.6	10.2	8.7	10.8	8.4	9.6
		S.d.	2.6	1.9	2.4	2.8	2.8	2.0
8-hr. days	7240	Average	8.4	9.5	8.6	9.6	8.1	9.4
		S.d.	2.8	1.3	2.6	2.9	2.9	1.3

It will be seen that the quality of the work as indicated by the average marks was slightly higher in the one-hour periods and slightly lower in the two-hour periods than in the four and eight hours. Speed of work, on the other hand, increased up to the four-hour periods and then remained fairly constant. It is highly probable that this increase of speed is, in the main, a practice effect, and that in this respect practice continued to influence results up to about half-way through the four-hour periods (see Table II).

Table II. *Average marks and times for success for six four-hour spells.*

Spell no.	Total no. of attempts		Mark	Time seconds
1	600	Average	8.0	11.5
		S.d.	2.7	2.5
2	600	Average	8.4	11.6
		S.d.	2.6	1.8
3	620	Average	8.4	10.0
		S.d.	2.8	1.7
4	620	Average	8.8	9.8
		S.d.	2.4	1.5
5	640	Average	8.8	9.2
		S.d.	2.5	1.0
6	620	Average	9.0	9.8
		S.d.	2.3	1.6

Table III. *Average marks for all 1st, 2nd, 3rd ... 15th sets of 40 attempts in eighteen four-hour spells.*

Sets of 40 attempts	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	14th	15th
Average mark for 18 sets	8.7	8.8	8.8	8.7	8.5	8.7	8.4	8.2	8.3	8.6	8.4	8.4	8.1	7.8	7.9
Std. dev. of set averages	0.2	0.7	0.7	0.6	0.5	0.5	0.8	0.6	0.5	0.4	0.4	0.7	0.8	0.6	0.9

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Taking all the four-hour spells together (including two on each eight-hour day), average marks were highest during the second and third sets of 40 attempts. At this stage a set took between fifteen and twenty minutes to perform, and so accuracy was highest from about fifteen minutes to one hour after the start. Here again the differences are small (10 per cent. between highest and lowest), but since the difference between the best and worst single sets performed at this stage was only 53 per cent., small differences in averages are worth recording. (See Table III.)

Table IV. *Marks and times for success for six eight-hour days.*

Day	Total no. of attempts		Mark whole day	Time whole day seconds	Mark a.m.	Time a.m. seconds	Mark p.m.	Time p.m. seconds
Mon.	1200	Average	8.5	9.6	8.8	9.9	8.2	9.2
		S.d.	2.6	1.5	2.3	1.6	2.8	1.4
Tues.	1180	Average	8.7	9.4	9.0	9.7	8.4	9.0
		S.d.	2.6	1.3	2.3	1.3	2.8	1.2
Wed.	1200	Average	8.5	9.2	8.7	9.4	8.3	9.1
		S.d.	2.7	1.3	2.4	1.2	2.9	1.3
Thur.	1240	Average	8.3	9.4	8.6	9.1	8.0	9.7
		S.d.	2.9	1.0	2.7	1.0	3.1	0.9
Fri.	1220	Average	8.2	10.1	8.3	10.2	8.1	9.9
		S.d.	3.0	1.2	2.9	1.1	3.1	1.2
Sat.	1200	Average	8.0	9.3	8.2	9.4	7.8	9.1
		S.d.	3.1	1.2	3.0	1.1	3.1	1.2

During the week of eight-hour days, accuracy was highest on the second day, Tuesday. On this day the number of attempts was smallest, but this might be expected since successful attempts naturally took longer than failures. After Tuesday average marks decreased regularly as the week went on. The afternoon marks were all lower than those for the preceding mornings and the standard deviations of marks were higher in the afternoons. On the first three and the last days, average time was greater in the mornings, on the fourth and sixth days it was greater in the afternoons.

Very broadly speaking, there appears to be a slight involuntary increase in the rate of work during the second as compared with the first half of a working spell, irrespective of the total length of the spell, and at the same time a slight decrease of accuracy. The increase in speeding (particularly in the eight-hour periods where the decrease in accuracy is greatest) is however so slight that it is rather difficult to believe that the decrease of accuracy is in any way due to this.

(b) *The effect of 'pauses.'*

On the eight-hour days an interval of an hour for lunch was allowed between the morning and afternoon spells.

Table V. *Average marks and time for last 40 attempts before and first 40 attempts after lunch pauses, expressed as percentages of average marks and average time for each day.*

	Percentages			
	Average marks		Average time	
	Last 40 before pause	First 40 after pause	Last 40 before pause	First 40 after pause
Mon.	99	104	85	89
Tues.	100	103	96	97
Wed.	98	104	105	105
Thur.	96	109	95	109
Fri.	97	103	105	100
Sat.	83	110	103	106
Average	96	106	98	101

It will be seen that the quality of work was, each day, higher immediately after the pause than before. Speed was greater once, equal once, and lower than before four times.

Rest pauses of five minutes with refreshments occurred about the middle of the four-hour days, and of each four-hour spell of the eight-hour days.

Table VI. *Five-minute pauses. Average marks and average time for last 40 attempts before and first 40 attempts after pauses expressed as percentages of average marks and average time for each spell.*

	Percentages			
	Average marks		Average time	
	Last 40 before pause	First 40 after pause	Last 40 before pause	First 40 after pause
Four-hour spells				
Spell no. 1	94	96	90	92
" 2	93	100	91	100
" 3	103	98	87	88
" 4	91	100	90	94
" 5	98	101	101	100
" 6	96	95	89	91
Eight-hour days				
Mon. a.m.	106	96	93	97
" p.m.	93	110	98	112
Tues. a.m.	100	96	91	102
" p.m.	104	102	104	109
Wed. a.m.	93	99	94	99
" p.m.	96	111	97	91
Thur. a.m.	91	98	95	96
" p.m.	105	101	101	101
Fri. a.m.	91	107	92	98
" p.m.	100	108	94	96
Sat. a.m.	101	103	103	104
" p.m.	94	106	98	96
Average for 18 periods	103	99	95	98

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Average marks for the first set of 40 attempts after the pause were better than for the last set before the pause twelve times and worse six times. Average time was longer fourteen times, unchanged once and shorter three times, Pearson's coefficient of correlation between increase of marks and increase of time being 0.3. For 18 pairs of observations a value less than 0.6 is scarcely significant.

There were also 42 incidental pauses lasting from one to three minutes for attention to stop watch, window, fire or electric light. Taking the averages for the 10 attempts before and after these interruptions, marks were worse afterwards 21 times, better 17 times and unchanged 4 times. Time was shorter 25 times, longer 16 times and unchanged once. Although accuracy on the whole suffered from these pauses, they were always extremely welcome, as they provided an opportunity of moving about.

(c) *The effects of feelings of tiredness.*

Feelings of tiredness were noted at certain times, but since they were not the original object of the study, probably they sometimes occurred without being recorded. During the practice and one-hour periods there was no reference to tiredness. At the end of the two-hour periods there were remarks such as 'rather tired' and 'ache in shoulders.' The subject felt tired at the end of every four-hour period (these were worked in the afternoons) and at the end of all afternoons of the eight-hour days. Tiredness was also noted at the end of the mornings of the last four days.

Table VII. *Average marks and time for 'tired' sets expressed as percentages of average marks and time for the spells in which they occurred.*

Four-hour spells	No. of 'tired' sets of 40 attempts	Percentage	
		Marks	Time
Spell no. 1	1	98	93
" 2	2	98	94
" 3	1	108	100
" 4	$\frac{1}{2}$	95	87
" 5	3	99	99
" 6	$2\frac{1}{2}$	98	98
Eight-hour days			
Mon. p.m.	5	94	108
Tues. p.m.	4	86	98
Wed. a.m.	2	94	102
" p.m.	3	88	100
Thur. a.m.	3	90	100
" p.m.	5	91	101
Fri. a.m.	$2\frac{1}{2}$	100	101
" p.m.	6	91	100
Sat. a.m.	7	93	100
" p.m.	3	95	100

The average mark for all 'tired' sets at the end of spells was 7.6 or 90 per cent. of the average mark for all the four- and eight-hour periods. Average time was unchanged. (See Table VII.)

(d) *Effects of 'conscious attitudes.'*

A matter of great interest in all psychological experiments is the possible effect upon results of varying 'conscious attitudes' on the part of the subject. As occurring in associative and higher thought processes generally, they have received a great amount of attention, but their precise effects in relation to specific sensory and motor processes need far more detailed study. An experiment of the type which I am now discussing affords abundant opportunity for this study but my remarks in regard to this must be taken rather as indicating points upon which future observation may well concentrate, than in themselves conclusive.

(1) *Cheerfulness and confidence.* At the beginning of a spell the subject was usually cheerful and after a short practice time became confident. Average marks were then high, over 9 for from one to five successive sets of 40 attempts, that is, for periods varying in length up to an hour and a half. Average time tended to decrease after the first set. After this beginning there was no uniformity, accuracy diminished but in an irregular fashion. Often a change of mood seemed to coincide with decreasing accuracy. Cheerfulness and accurate work were certainly associated, but whether as cause and result or *vice versa* this experiment did not prove.

(2) *Devices to maintain cheerfulness.* It was found that certain devices, consciously adopted, could assist in maintaining cheerfulness. The most useful of these appears to have been singing. This started on the

Table VIII. *Average marks and time for sets during which the subject sang, expressed as percentages of average marks and time for the spells during which these sets occurred.*

Period	No. of 'singing sets'	Percentage	
		Marks	Time
Tues. a.m.	4 $\frac{1}{4}$	102	102
" p.m.	5 $\frac{1}{4}$	106	113
Wed. a.m.	4 $\frac{1}{2}$	102	104
Thur. a.m.	5	105	101
" p.m.	6 $\frac{3}{4}$	103	102
Fri. a.m.	6 $\frac{1}{2}$	105	100
" p.m.	4 $\frac{1}{2}$	106	104
Sat. a.m.	5 $\frac{3}{4}$	105	101
" p.m.	4	105	107
Total	46 $\frac{1}{2}$	Average 104	104

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Tuesday morning of the week of eight-hour periods, and for the rest of the week, the subject sang to herself during each spell except that on the Wednesday afternoon, for periods of time varying from one to two hours.

It will be seen that during these singing periods accuracy was slightly higher and time slightly longer than the general average. But accuracy did not remain at the same level throughout a singing period, it tended to rise gradually and then fall again when the subject was getting bored with singing. Time varied irregularly. (See Tables VIII and IX.)

Table IX. *Example of fluctuations in marks and time during a 'singing' period. Friday, p.m. (Averages for 10 attempts.)*

Sets of 10 attempts	1st	2nd	3rd	4th	5th	6th	7th	8th	9th
Average marks	7.9	7.4	8.2	9.7	9.4	8.8	10.0	8.4	9.6
Average time	9.9	9.8	9.8	10.7	10.4	10.8	10.1	10.4	10.1
Sets of 10 attempts	10th	11th	12th	13th	14th	15th	16th	17th	18th
Average marks	8.8	9.1	9.1	8.4	8.0	9.4	6.9	7.3	7.2
Average time	9.8	10.3	9.5	10.1	9.8	10.1	12.0	10.4	11.8

Another device to maintain cheerfulness was definite concentration on speed, as providing a special interest. This was recorded 12 times with the following results:

Table X. *Average marks and time for periods of concentration on speed expressed as percentages of average marks and time during the spells in which these periods occurred.*

	No. of attempts in period of concentration	Percentage	
		Average marks	Average time
Two-hour spells			
Spell no. 5	8	102	63
" 5	7	100	71
Four-hour spells			
Spell no. 2	10	104	86
" 3	20	102	96
" 4	25	105	85
" 6	50	90	84
Eight-hour days			
Tues. a.m.	40	101	87
" p.m.	80	93	96
Wed. p.m.	40	101	92
Thur. a.m.	40	98	87
" p.m.	15	100	89
Sat. p.m.	40	99	86
Total	375	98	88

(3) *Boredom and annoyance.* In spite of the above devices to maintain cheerfulness, the subject was unsuccessful in amusing herself the whole time. She experienced periods of severe boredom, especially during the

eight-hour days. Three distinct types were reported: (i) angry, (ii) 'nightmare' or compulsive, (iii) passive.

(i) The subject raged at the experiment and was annoyed with herself for working badly. The mood was present on six occasions, and each time the average mark was lower than the average for the spell in which it occurred.

Table XI. *Average marks and time during 'angry' sets expressed as percentages of average marks and time for the spells in which they occurred.*

Eight-hour days	Set	Time after	Percentage	
			Average mark	Average time
1 Wed. p.m.	5	1 $\frac{1}{4}$ hrs.	92	100
2 Thur. a.m.	7	1 $\frac{1}{4}$ "	91	93
3 " p.m.	15	4 $\frac{3}{4}$ "	81	96
4 Fri. p.m.	13 and 14	4 $\frac{1}{4}$ "	88	101
5 Sat. a.m.	13	4 $\frac{1}{4}$ "	80	105
6 " p.m.	2	20 min.	94	107
Average for 'angry' sets			88	100

(ii) The 'nightmare' or compulsive feeling can best be described by quoting the introspections recorded the first time it appeared. "It all feels rather like a bad dream; I have *got* to do this silly thing and go on doing it. Something outside is driving me on against my will." This feeling was present during the first set on the Friday morning and again during the fourth. It was overcome by suggestion and an effort to improve which will be discussed later. For those sets the average mark was 92 per cent. and time 100 per cent. of the averages for the whole spell. The feeling was also present during the 13th set on Friday afternoon but then changed into the angry type of boredom which is recorded above.

It is interesting at this point to compare some results of experiments by E. M. Smith and F. C. Bartlett on "Listening to Sounds of Weak Intensity¹." Subject S reports on p. 154: "I had a fit of absolutely choking rage... This lasted for a while and I could not attend at all. There was an extraordinary feeling of being thwarted, accompanied by a horrible feeling of helplessness which took all my attention away from the sound, and would have prevented my hearing it anyway." The report continues: "In spite of these apparently unfavourable moods, the observer's performance on this occasion was approximately normal. In the Guidit experiment the subject's performance during moods of anger or 'nightmare' was definitely below normal. The difference in results may possibly be due either to the fact that a motor and not a sensory response

¹ This *Journal*, March 1920, x, pts. 2 and 3.

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Table XII. *Average marks and time during 'bored' sets expressed as percentages of average marks and time for spells in which they occurred.*

Two-hour spells	Set	Approx. time after start (hrs.)	Percentage	
			Average mark	Average time
Spell no. 2	5	1½	99	103
Four-hour spells				
Spell no. 3	7	2	84	100
" 4	9	2½	93	92
" 5	6	1½	107	99
" 6	5	1¼	97	105
" 6	8	2	96	97
Eight-hour days				
Tues. a.m.	9	2½	96	103
Thur. p.m.	3	1	95	95
Fri. p.m.	8	2	100	104
Sat. a.m.	4	1	102	94
" p.m.	5	1¼	100	93

Table XIII. *Average marks and time for sets of 10 attempts before and after special efforts expressed as percentages of averages for the spells in which they occurred.*

A. Marks.

Four-hour spells	Approx. time after start hrs.	Average for last 10 before effort	Average for first 10 after effort	Average for second 10 after effort	Average for third 10 after effort	Introspection recording effort
Spell no. 2	2	83	92	69	104	Effort to wake up and do better
" 3	1½	89	101	93	90	Pulled myself together
" 3	2	87	107	92	95	Suggested confidence
" 4	1	102	103	101	108	Tried extra hard; wanted 10 out of 10
" 5	3¼	89	107	94	95	Suggested confidence
Eight-hour days						
Mon. a.m.	2	79	122	107	89	Determined effort
Fri. a.m.	1¼	95	93	105	105	Suggested confidence after nightmare feeling
Sat. p.m.	3¼	90	96	86	128	Special effort. Last set of experiment
Total average		89	103	94	102	

B. Time.

Four-hour spells	Approx. time after start hrs.	Average for last 10 before effort	Average for first 10 after effort	Average for second 10 after effort	Average for third 10 after effort	Introspection recording effort
Spell no. 2	2	99	134	132	87	See above
" 3	1½	98	119	107	112	
" 3	2	82	98	83	91	
" 4	1	94	96	111	104	
" 5	3¼	99	99	99	102	
Eight-hour days						
Mon. p.m.	2	104	97	92	93	
Fri. a.m.	1¼	102	104	89	89	
Sat. p.m.	3¼	102	101	114	119	
Total average		98	107	103	100	

Table XIV. *Average marks and time for sets of 40 'drowsy' attempts expressed as percentages of averages for spells in which they occurred.*

	Set	Approx. time after start hrs.	Percentage	
			Average mark	Average time
Two-hour spells				
Spell no. 4	3	1½	101	97
Four-hour spells				
Spell no. 2	5	1½	98	106
„ 3	6	1¾	99	104
„ 3	11	3	90	88
„ 3	12	3½	98	95
„ 3	13	3½	100	95
„ 4	8	2½	95	101
„ 4	14	3¾	99	94
„ 5	11	3	101	100
„ 6	4	1½	107	103
„ 6	9	2½	98	92
Eight-hour days				
Mon. a.m.	11	3	99	101
„ a.m.	12	3½	102	98
„ a.m.	13	3½	102	89
„ p.m.	5	1½	104	91
„ p.m.	8	2½	91	97
„ p.m.	9	2½	96	101
Tues. a.m.	10	2½	93	99
„ p.m.	10	2½	100	108
„ p.m.	11	2¾	100	109
Wed. a.m.	6	1¾	103	98
„ p.m.	7	2	106	102
Thur. p.m.	6	1¾	100	92
Fri. a.m.	3	1	98	98
Sat. p.m.	9	2½	92	98
Total average			99	98

was required in the latter experiment, or to the fact that while performing Guidit the subject had a very fair idea of the goodness and badness of her work. Both explanations are extremely tentative and based on insufficient data, but further investigation should help to solve the problem.

(iii) As well as the moods already described, many periods of a more passive kind of boredom were experienced in the course of the Guidit experiment. Many of these were accompanied by feelings of tiredness and of general drowsiness, and as such are detailed elsewhere in this report. Periods of this passive boredom, in which tiredness and drowsiness were not reported, are recorded above. (See Table XII.)

The average mark for these sets is 97 per cent. and the average time 99 per cent. of the averages for the spells.

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(4) *Effort to improve.* The subject was trying to work accurately the whole time, but a special effort to improve was recorded eight times. (See Table XIII.)

(5) *General drowsiness.* This condition was frequently reported, it was usually described as 'Bored and sleepy.' Sets where it occurred unaccompanied by feelings of tiredness are detailed above. (See Table XIV.)

(e) *The effects of 'distractions.'*

Two kinds of distractions were observed: (i) when there was a noise outside, usually of a mowing machine, which lasted through a whole set, and (ii) distractions which lasted a very short time; there were also two cases of special interest distractions.

(i) Table XV. *Average marks and time for sets of 40 attempts during which the subject was partially distracted, expressed as percentages of the averages for the spells in which they occurred.*

Four-hour spells	Set	Approx. time after start hrs.	Percentage	
			Average mark	Average time
Spell no. 4	5	1 $\frac{1}{4}$	106	112
Eight-hour days				
Wed. a.m.	7	1 $\frac{3}{4}$	105	95
„ p.m.	4	1	107	110
„ p.m.	9	2 $\frac{1}{4}$	96	103
Thur. p.m.	8	2 $\frac{1}{4}$	105	101
Total average			104	104

The results for these sets average the same as those for sets during which the subject sang to herself (see Table VIII). In both cases atten-

(ii) Table XVI. *Average marks for the 10 attempts before and after momentary distractions, expressed as percentages of averages for spells in which they occurred.*

Four-hour spells	Set	Approx. time after start hrs.	Average mark		Average time		Cause of distraction
			% for 10 before	% for 10 after	10 before	10 after	
Spell no. 3	8	2	95	99	91	97	Aeroplane outside
„ 5	9	2½	114	107	97	101	Someone coming into the room
Eight-hour days							
Mon. a.m.	6	1½	91	105	101	96	Someone coming into the room
Wed. a.m.	13	3½	99	115	103	102	Church bells outside
„ p.m.	8	2	86	111	93	109	Someone coming into the room
Total average			97	107	97	101	

tion was partly occupied by other things than the performance of the task and accuracy was slightly raised and speed slightly lessened. The distractions were disliked rather than liked.

The above distractions seemed to have a good effect on accuracy. It is not recorded whether the aeroplane and church bells were liked or not, but the interruptions of people coming into the room were very welcome indeed.

The first 'special interest' distraction was on the Monday afternoon of the week of eight-hour days. The subject was entertained by a drawing by an unknown hand on her scoring sheet, and marks and time for that set were 109 per cent. and 91 per cent. respectively of the averages for the spell. The other occasion was on the Saturday afternoon when the stopwatch refused to stop at the first press and simultaneously returned to zero at the second. This meant that it had to be observed very carefully at the end of each attempt and it created a new interest. Average marks rose from 99 per cent. for the previous set of 40 attempts to 110 per cent. for the following set, but the interest soon lost its novelty and average marks for the next set fell to 94 per cent. Average time for the three sets in question were 86, 90 and 90 per cent. of the average for the spell.

V. CONCLUSIONS.

The following general, but tentative, conclusions may be drawn:

1. That in all periods of one hour and upwards accuracy tended to decrease in the second as compared with the first half of the spells of work, the relative decrease in accuracy apparently increasing in spells of above one hour, but thereafter remaining fairly constant up to spells of eight hours. No longer spells than eight-hour periods were worked.

2. That there appeared to be a small involuntary increase of rate of work during the second as compared with the first halves of the spells irrespective of the length of the spell.

3. That in general accuracy improved slightly after prescribed pauses, more or less irrespective of their length, and seemed to decrease slightly after unexpected pauses enforced by some disturbing external circumstance.

4. 'Feelings of tiredness' were recorded in two-hour spells and above, and were almost consistently accompanied by a fall in accuracy (average fall about 10 per cent.), but by practically no change in the rate of work.

5. An attempt was made to record variations of 'conscious attitude' throughout the working spells, and it seems possible to make the following suggestions:

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Cheerfulness and confidence are fluctuating attitudes, commonly marked at the beginning of a spell and thereafter varying irregularly. Whenever recorded they seem to mark a period of increased accuracy.

Special devices adopted to maintain cheerfulness may have a direct effect on the accuracy of the work. Singing, for instance, invariably appeared to improve accuracy and in general slightly to decrease rate of work.

Concentration on speed for short spells, adopted to maintain cheerfulness and confidence, generally resulted in increased accuracy as well as in increased rate.

Boredom was reported to fall into three types: (1) angry, (2) 'nightmare,' compulsive, (3) passive. During spells in which the first occurred there was an average 12 per cent. loss of accuracy, but no decrease of rate. The 'nightmare' type is where the operator has a hopeless feeling of being 'driven' to work by some force outside herself. Here again is loss of accuracy, but no decrease of rate. In passive boredom a much smaller loss of accuracy was observed and also a very slight decrease of rate.

General drowsiness seemed to produce a slight fall of accuracy and a slight increase of rate.

6. Distractions were recorded and were divided into two classes, *i.e.* outside distractions produced by external continuous noises, and momentary distractions of a special character, such as the coming of individuals into the experimental room. The first seemed slightly to increase accuracy and decrease rate of work: the second to increase accuracy more but to have little effect on the rate of work.

In conclusion I wish to express my thanks to Mr Bartlett for his help and advice.

(Manuscript received 21 September, 1927.)

THE CREATIVE ACTIVITY

INTROSPECTIVE EXPERIMENTS IN MUSICAL COMPOSITION

BY EVELYN BENHAM.

- I. *Introduction* (pp. 59-60).
- II. *Method* (pp. 60-61).
- III. *Preliminary experiments* (pp. 61-62).
- IV. *Mood sets* (pp. 62-63).
- V. *Summary* (pp. 63-64).
- VI. *Conclusions* (pp. 64-65).

I. INTRODUCTION.

THESE experiments in regard to the creative activity have been undertaken after a study of the habits of composers as noted in letters and biographies, and after a consideration of any comments made on the subject by psychologists interested in the problem specifically or as a general problem. The tendency in modern musical biography is to note what psychological attributes the habits of composers evince. This might be extended to a consideration of the extent to which these modes of procedure are manifested by different composers. It certainly leads logically to a consideration of the conditions which the composer finds most suitable, what occurs in his mind prior to writing, what happens *while* he is writing, the handling of his initial idea, and of his methods of writing in general. The study of records reveals undeniably the conscious recognition of preliminary activity, either of short or long duration. It also reveals the manifestation of kinaesthetic activity during the act of composing, the necessity for keen auditory imagery, and the questionable need of visual imagery. The study of articles in current journals reveals a general interest in the problems of the pre-existing whole, imagery with reference to art and literature, the activity of the subconscious, and the intellectual and emotional content of the inspirational experience. In regard to the subconscious, emphasis has been laid on the activity which must lead up to the 'flash' or 'spark' of inspiration. Dessoir, Prescott, Lowes, and Wallas have dealt with this subject, and Howes

has commented on the musical aspect. Recent experiments on the creative process in art have been made by Paulsson.

The only possible scientific approach to the problem of creative activity in music has appeared to be the observation of imagery. The task is to ascertain just what auditory imagery passes through the composer's mind in a stated amount of time preceding writing, the evidence for visual imagery either of the notes or an extraneous nature, and any evidences of kinaesthetic or organic imagery. The nucleus is the initial imagery. The centre of problematical intensity is the exact point where the initial idea emerges. The focus then before starting these experiments was imagery, and the need of establishing some method for analysing that imagery.

Subsidiary results which might accrue are both musical and psychological. An awareness of conditions by the composer might lead to the possibility of procuring these conditions consciously. An awareness of his type of imagery might facilitate his production. Psychologically the study of the aesthetic experience as such might be aided by this particular focus.

II. METHOD.

In attempting these experiments, quiet surroundings were found. Pencil and ordinary manuscript paper with the treble clef sign were used. No bar lines, key or time signatures were indicated during the writing of the melodies.

The purpose in starting the experiments was to notice the general conditions facilitating writing, and to try to note the appearance of the initial idea. As they were continued, attention was concentrated on imagery of any kind connected with the initial idea. Any imagery observed in the minute preceding writing was noted for comparison. An effort was made to write with as little conscious effort as possible, so as to be unaware of the fundamental rhythm or key of the melodies, while avoiding pure automatism. As a result, each melody had to be analysed in order to make it understandable or usable.

Notes were made during each experiment and at the conclusion of each one. It was found advisable to divide these notes into three sections. First, any observation about the preceding minute; second, notes on the writing of the idea; third, notes on the manipulation of the melodies after writing, *i.e.* addition of bar lines, key signatures, etc. Analysis often revealed unusual phrase balancing or time patterns of great variety. In the early experiments, any imagery was noted, with the emphasis on

the auditory. Later experiments showed the need of individual analysis of visual, organic, kinaesthetic, in order to separate somewhat these types from their affiliation with the ordinary experience. An attempt was made to restrict the preceding minute to that time valuation, and to gauge roughly the time used for melody writing.

III. PRELIMINARY EXPERIMENTS.

Twelve melodies were written between September 14, and October 15, 1926. The observation in these early experiments was fragmentary. Visual imagery, it is stated, was not noticeable. "In one instance only is there no first impression of a nucleus of a few notes. I had no idea of the kind of melody that was coming. The writing of the first note—its place and kind—was definite, also the metronomic speed." "The melody does not come on any denoted instrument." "Kinaesthetic activity was not noticed." "The melodies are forgotten immediately after writing."

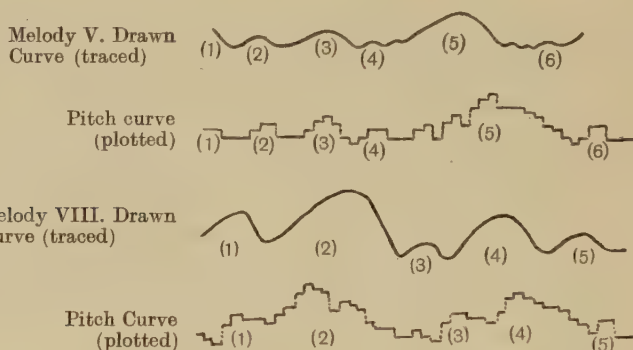
The second experiment consisted of six melodies written between October 21, and November 19, 1926. There was nothing exceptionally different in the writing or in the analysis, the latter being still vague. "It would seem—that the lack of recognition of tonality other than a note as tonal centre, facilitates the smooth writing under the specified conditions, as does the obscured indications of the rhythmical values."

The third experiment took place in another locality from January 30, to February 19, 1927, covering ten melodies in all. The advance in analysis occurs in the recognition of "auditory imagery while writing, as though singing." This 'singing' is apparently a combination of ill-defined kinaesthetic and auditory imagery—more auditory than kinaesthetic. The second innovation occurs in statements intimating a vague sort of organic activity, "a muscular feeling of a rhythm rising and falling"—"the writing seemed to afford rhythmical satisfaction." At the conclusion of the experiment, pitch curves were drawn of three or four of the melodies. No. VI, where the 'muscular feeling' was particularly noted, looked as follows:



In the plotting, the single space horizontally indicates a quarter tone, vertically, two whole steps. These curves suggested the problem of the relation of rhythmic sensation to the pitch curve, and whether there were not some preliminary rhythmic curve which was motor-muscular.

The fourth experiment had nine melodies from February 20, to March 16, 1927. An attempt was made to clarify the preliminary curve. A piece of plain paper was taken, the line drawn with the eyes shut, and the paper placed out of sight before the writing of the melody. In graphing the pitch curve of the melody for comparison, the horizontal single space equalled one eighth note, the vertical single space was the equivalent of two whole tones, accidentals not being indicated in this rough plotting. Little imagery of any kind seems to have been connected with the drawing of the curves. In No. IV, however, there is "auditory imagery at once when the preliminary curve is drawn—notes coming with the curve." The question immediately arises as to whether the rhythmic and auditory material exist in conjunction prior to writing. The notes regarding the melody writing show that memory of the general nature of the drawn curve is retained from the muscular action, but not the details. For comparison, the preliminary drawn curve was traced above the plotted pitch curve of the melody immediately following. Nos. V and VIII are reproduced, the various sections of the curves being numbered for comparison.



The most significant items in regard to the lines are first, the preliminary drawn curve, and second, the way in which the smaller details of the drawn curve and pitch curve tally. This was noticeable to a lesser degree in all of the melodies of this experiment.

IV. MOOD SETS.

A correlative series of experiments was made from January 7, to January 19, 1928, in an attempt to observe the kinaesthetic reactions. Certain mood sets were chosen, with the idea that the physiological set

as deliberately imposed would influence the mood and hence the melody as the expression of the mood. The following sets were planned:

- | | |
|---------------------------|--|
| (1) Dejection, Lassitude. | Limp body; passive mind; drooping of arms, legs, head. Relaxed facial muscles. |
| (2) Peace, Calmness. | Relaxed body; passive, but sustained, mental attitude; drooping, no tension. |
| (3) Joy, Elation. | Straight back, head tilted back; eyes lifted; note mouth muscles; hands clasped; note shoulders. |
| (4) Energy, Decisiveness. | Purposed tension of body; hands slightly clenched; head bent forward with body; mind concentrated. |
| (5) Sorrow, Suffering. | Head on hands; elbows on knees; tension of mind and body. |
| (6) Super-restraint. | Cf. (4). Tension of mind and body, inhibiting action, but with similar bodily appearance. |

The physiological details were amplified when the experiments were made, and notes made as to the maintenance of the set, and the mood. Nos. I and VI were tried twice, No. II three times for better musical results. In every instance, the physiological set as planned was achieved in the preceding minute with a mood of greater intensity than planned. After writing, notes were made regarding the maintenance of mood and set during the writing, and further notes were made regarding manipulation of the melody, and its value with reference to the mood. It was reported that the mood set was maintained in every writing, although the physiological conditions necessarily had to be altered. In every instance the melody was a consistent expression of the mood desired and these expressions were clearly differentiated. The precautions used in writing the previous experimental melodies were maintained here, and notes made on any awareness or lack of it while writing. The most successful was No. V, where the mood imposed was very unlike the preceding state of mind, and where the musical results were found to tally in a manner not realized while writing them.

The possible conclusions are (i) that certain motor adjustments can be deliberately imposed, (ii) that these adjustments can be sustained at will for a certain period, (iii) that these adjustments produce simultaneously a mood correspondence which may have been wholly lacking at the outset, (iv) that this set super-imposed affects the content of the output—in this case, the musical expression in a melody.

V. SUMMARY.

The average rate of speed in writing the melodies was 60–70 seconds, the average length of melody being nine measures. The tonality was consistently minor, *d* and *e* predominating. Phrasing and time patterns were irregular. The average time was $\frac{4}{4}$, although there was frequent

use of $5/4$, $6/4$, and $7/4$. One third of the melodies were satisfactory musically, and have since been used. The melodies were evenly divided as to tempo. The reports on visual and kinaesthetic imagery were not definite. Auditory imagery was reported in the 'singing.' The curves presented a new problem in imagery. To what extent is auditory imagery a fluctuating phenomenon? To what extent is combined kinaesthetic and auditory imagery essential? What suggestive value is existent in the curve data?

In the mood set experiments, the average rate was 60–70 seconds where noted, the average length of melody being twelve measures. There were six melodies of major tonality to four of minor. The average time was $4/4$, except for No. VI, 2, which was $6/4-4/4$ alternately, and No. V which was patterned $5/4-3/4-3/4$, as analysed after writing. No. V was particularly notable in the maintenance of a mood not at all present before the experiment, and in the effect of that mood upon the music.

VI. CONCLUSIONS.

These investigations indicate that definite evidence can be obtained regarding the preliminary activity in composing. There is evidence of motor sensations which may be allied with the rhythmical aspects of the music. Auditory imagery is apparently present, for which trained observation is necessary. It is a question whether this auditory imagery is present previously as a major item, or whether the observation of it is the focussing upon one aspect of an imagery which is a synthesis. Or it may be that the various kinds of imagery can be more easily observed in the preliminary period, and the closer synthesis lies in the writing of the initial idea. Certainly the mood set, to be effective, should be established in the preliminary period.

In regard to the initial idea, the auditory imagery is strongest at the emergence of the idea, strengthening the thesis that that point is the centre of problematical intensity. Observation should be made regarding kinaesthetic and other imagery, to see whether they also are strongest at that point. It would appear that the control of mood set is possible and that a mood pre-established can be maintained for a stated time as desired.

Ultimate value might therefore seem to be possible for the musician in the control of subconscious activity, of mood sets, and in the development of an awareness of mood, and of imagery. This activity would sharpen his musical ideas. For aesthetics, there would seem to be a connection between mood sets and musical appreciation, and the need of observation of imagery.

In these experiments, the intention is to clear up correlative experiments, such as experimenting with visual imagery, before analysing the auditory as such. After the visual and the auditory, an attempt will be made to cover the various aspects of imagery in a single series. Then should follow the objective attack, dealing both with the phases of imagery, and the grouped series, in order to establish the validity of the introspection.

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RESULTS FROM SOME NEW TESTS OF PRACTICAL ABILITIES

BY W. VICKERS AND V. H. HOSKIN.

(*From the Department of Education, University of Sheffield.*)

- I. *Introduction* (p. 66).
- II. (a) *Description of tests* (pp. 67-72).
(b) *Children and workpeople tested* (pp. 72-73).
- III. *Results and conclusions* (pp. 73-80).
- IV. *Conclusions summarized* (pp. 80-81).

I. INTRODUCTION.

ALL tests of practical ability involve in varying degrees at least two types of activity, namely, intelligence and bodily skill. At one extreme there is such a test as the Picture Completion Test, where meanings must be arrived at through pictures with or without the use of words, and where the reasoning is based upon these meanings so arrived at. The manual skill needed for placing the pictures in the recesses is (from the standpoint of its grade or the time it occupies) both small and unimportant. The other extreme—where the intelligence needed for grasping the problem presented by the test is of a low order, and yet where the bodily dexterity demanded is of a high order—is represented by such a test as the “Moving Line” Test to be described.

The tests of practical ability here to be considered were evolved originally in order to carry out vocational guidance experiments in two manual occupations, namely, machine file-cutting (a moderately skilled occupation) and machine tool-making (a highly skilled occupation). These occupations were carefully studied and analyses made of their psychological requirements. Tests were then devised in accordance with these analyses. Certain of the tests were intended to measure specific abilities; others had as their object the measurement of a postulated more general practical ability.

In order to standardize the test material and procedure the series was tried out on 115 thirteen-year-old school children. When the tests had been made sufficiently reliable they were given to workpeople.

II (a). DESCRIPTION OF TESTS.

Obstacle Test. The problem in the Obstacle Test was easy to grasp, but the solution involved a moderately high level of reasoning. If (as James and Professor McDougall argue) the essential feature of reasoning is the sagacious selection of relevant provisional solutions to problems, then, whereas the usual type of reasoning test gives scope for the exercise of reasoning on the imaginative plane, the Obstacle Test gave it scope on the practical plane. The test involved this sagacious selection of cogent hypotheses but on the level of practical trial and error. Delicacy of touch, dexterity in manœuvring materials into position, clean cut movements and persistence in the face of repeated failures, were further minor factors present in this form of the test, although they can be made to play a large part in other forms.

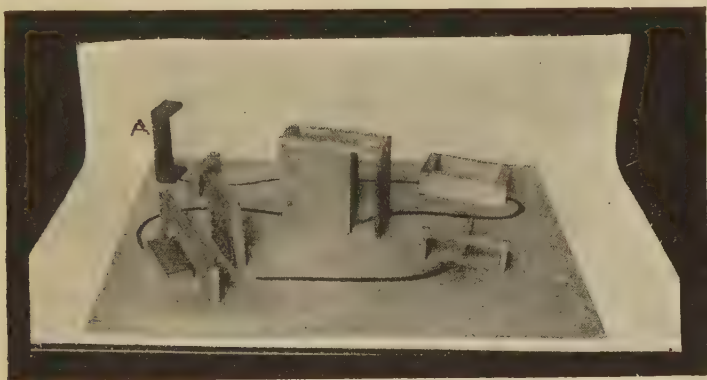


Fig. 1.

The Obstacle Test (see Fig. 1) consisted of a board on which was mounted a series of obstacles graded in difficulty. Through these obstacles the subject was required to manœuvre the wooden object (A). The subject was told: "This is an obstacle race, and this is the piece which has to be taken through them. It first goes through this one and then this, etc." (The experimenter indicated the order of the obstacles and the entrances and exits. A line on the board served to remind the subject of the order in which the obstacles were to be taken.) The experimenter then continued: "It does not matter how you take the piece through so long as you do not use force. As a matter of fact, when you discover the correct way the piece will slip through quite easily. Go through the obstacles as quickly as you can. Now begin."

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The times taken to overcome each obstacle were noted. The second and third obstacles in the figure were regarded as one obstacle. A maximum time limit was set to each piece for practical reasons. These limits corresponded for each obstacle to the points 1.5σ of a group of thirteen-year-olds. They were as follows: (1) 60, (2) 70, (3) 120, (4) 100, (5) 60—all expressed in seconds. The shortest total time recorded for school children (120 cases, boys and girls, $13\frac{1}{2}$ average age) was 54 seconds, and the longest time 396 seconds. The time for each obstacle was converted into a weighted score. The maximum scores allotted to the individual obstacles were made inversely proportional to the percentage of the failures in the above group at each obstacle at the end of 20 seconds. These percentages were: (1) 15, (2) 19, (3) 90, (4) 86, (5) 90. Tables, for converting to test scores the time scores of each subject for each obstacle, were made by multiplying the times, expressed in terms of the standard deviation of the above group of children, by the appropriate weight.

The 'reliability' (or, as it is called later, the 'consistency') coefficient obtained from this group by this test, was 0.80 ± 0.04 .

If this type of test is found to be applicable to other industrial occupations, then it seems worthy of an effort at systematic standardization for the purposes of vocational guidance. It is being further exploited.

Balancing Test. The materials required for this test are a pointed ink eraser to act as a pivot, a Gestetner "black metal plate," and some weights suitable for attaching to the edges of the plate.

The subject was required to balance the plate horizontally on the pivot by moving the plate by means of the finger tips which were placed under the plate. In order that the plate might not be balanced by an estimation of the geometrical centre, two weights were attached to the edges. This had the effect of moving the balance point from the geometrical centre. Variations of the test have been made, for the purpose of practice and for the purpose of obtaining a reliability coefficient, by adopting the simple expedient of turning the plate round or upside down. A practice and four trials in all were given.

On the basis of experience gained by testing the above thirteen-year old group, a time limit of 90 seconds was set for each trial, and a scoring table was drawn up. This table expressed time scores in terms of the standard deviation of the group.

A 'reliability' (or 'consistency') coefficient of 0.65 ± 0.05 was obtained on testing the above group.

It was not intended that this test should measure any specific ability, since the movements it demands are in no way comparable to those found either in Engineering or School Handwork. This test probably evaluated certain temperamental traits such as care and caution, besides being a test of practical ability. The subjects soon discovered that the best way of balancing the plate was by moving it quite a small distance at a time, and after each move to make a 'trial balance.' This served as a guide to the direction of the next small movement to be made. Thus care played an important part in the degree of the subject's success.

Inverse Formboard. A modification of the usual simple formboard was adopted. The chief difference lay in the features that the forms were mounted on larger discs and they were the mirror images of the recesses. Thus each form when viewed by the subject appeared as the mirror image of the recess into which it was to fit, but when the form was inverted ready to be placed in the recess, the disc mount hid the form from view. Thus any trial placement had to be made by actual movement, it being impossible to effect the trial by simple direct visual comparison.

The number of moves made by the subject were recorded in addition to the time taken in completing the test. Time scores and move scores were again transmuted into composite standard scores of the thirteen-year old group.

Moving Line Test. It appeared desirable to obtain a test which would include varied sweeping wrist and short arm movements. Tests of the formboard and pegboard type allow of no varied movements, but require a number of movements of approximately the same description. These are, moreover, jerky and straight as opposed to the natural flowing movements of all skilled performances. Hence accurate sweeping movements which required also a steadiness of hand have been embodied in a 'moving line' test. The aid of vision to a test of this description is apparent, but the evidence of introspective data points also to the material help of kinaesthetic imagery.

In the test the subject was required to trace a wavy copper line which appeared on a revolving drum. The tracing was done through a fixed aperture by means of a metallic pencil. So long as the subject was able to trace the line perfectly, electrical contact was maintained and the movement of a chronoscope was suspended. But as soon as the subject's pencil left the line electrical contact was broken and the clock began to register. Thus whilst the subject kept to the line the clock did not register, but the oftener he left the tracing the longer was the time

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recorded. The final time reading of the chronoscope gave the score directly. The lower the time score, of course, the better the test score.

A copper wire of square cross section was soldered on to the curved surface of an ordinary smoke drum and the space between the lines of wire was filled with ebonite to the level of the copper. When finally turned and polished, there was displayed on a smooth surface of ebonite a wavy copper line.

The design of the copper line had been previously determined by experiments on school children, using a line drawn on a paper which covered the drum. The degree of difficulty of the pattern, the speed of the drum, the width of the line, and the dimensions of the aperture,

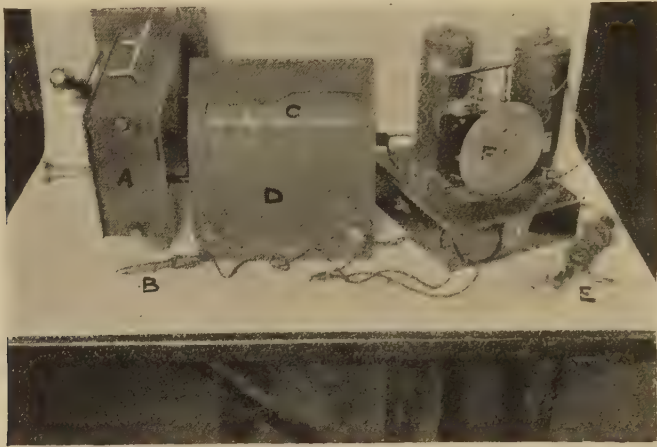


Fig. 2.

were in this way decided upon before the drum was actually made. Two types of lines were used (Moving Line A and Moving Line B). Moving Line A consisted of long waves. This test was one which estimated skill in the movement of the wrist and forearm. The Moving Line B, however, consisted of short curves of small amplitude. The drum, moreover, revolved at a lower speed than in the former test, and so made the test longer. The movements necessary to follow the line were simple enough, but it was necessary to have no straying of attention.

Fig. 2 shows the layout of the apparatus. *D* was a screen with an aperture through which a part of the drum *C* could be seen. *A* was the clockwork mechanism which drove the drum. The pencil which the subject held is indicated by *B*. It was connected to the D'Arsonval

chronoscope *F* by an insulated cable. An auxiliary control of *F* was possible with a key *E*.

The test started with the chronoscope's action suspended because the key *E* was caused to make contact. Afterwards the key, which was normally open, was dispensed with until the end. The standardized instructions were: "Here is a revolving drum which has on it a wavy copper line. In a moment take this pencil and trace the line as carefully as possible. The method of holding the pencil is this." (The experimenter demonstrated. The pencil was so held that the distance between the fingers and the contact point was constant for all. This was done by having a small groove in the pencil.) The experimenter then continued, whilst demonstrating: "Now place the pencil on the black part of the drum, pressing with medium strength, and wait for the line to come into view. As soon as the line appears, transfer the pencil on to it from the black, and then try to keep on the line all the time, taking care not to take the pencil off the drum."

When next the line appeared the subject started the test. The line was so designed that the first part was nearly straight in order to give the experimenter the chance of starting the clock as soon as it was seen that the subject had begun the tracing. Gradually the curve became more difficult, but it finished as it began in a manner easy to trace. Finishing thus had two advantages, one being that it was easy to stop the chronoscope, and the other that the attitude of mind left with the subject was that, at any rate, he was not a complete failure.

Each line was traced three times, the first one counting as a practice. When tried out on a class of 36 schoolboys (mean age $13\frac{1}{2}$) the correlation between the second and third attempts was found to be $+0.88 \pm 0.06$. Thus the test is consistent.

Area Sorting. This was devised to test the specific ability necessary to a certain extent in machine file-cutting. A pack of cards was required to be sorted according to the areas of the rectangles described on them. The consistency coefficient when 30 cases were tested was found to be 0.84 ± 0.07 .

Aiming Test. This was an adaptation of the test described by Whipple. Instead of a series of crosses a zigzag line was substituted. The subject aimed at each corner of the diagram zigzag to the beat of a metronome. The zigzag was of such a shape that the subject could see, when hitting one target, the position of the next target. The consistency coefficient was 0.71 ± 0.09 .

Substitution Test. This was an adaptation of the Woodworth Wells

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Substitution Test. Cards bearing geometrical figures had to be sorted into compartments. This again was a test for a specific ability useful in file-cutting.

Abstraction. It was found in preliminary work that the usual types of intelligence tests involving, as they do, responses usually in words, are very unsatisfactory when applied to adult subjects. An intelligence test was therefore designed requiring card sorting as the only response. The subject had to ascertain the several similarities common to geometrical and other drawings on a card, and place it in one of many compartments which bore specimen cards on which were drawn some more figures having common features. The cards were to be sorted into the appropriate compartments. The consistency coefficient was 0.69 ± 0.05 .

Judged Reaction Test. The subject was required to stop an ordinary D'Arsonval Electric Chronoscope when the finger of the latter came opposite to a definite mark on its face. This was effected by a hand switch. The errors made in 40 attempts to stop the clock at the required mark were noted, and the mean of these transmuted into standard scores.

The test was devised to measure the accuracy with which a subject could stop a moving object at a certain place.

A consistency coefficient of 0.70 ± 0.09 was obtained.

A Slide Rest Test. A somewhat similar type to that used by Mr Max Tagg was adopted¹. A practice was allowed.

The consistency coefficients were found to be: for accuracy 0.92 ± 0.02 ; for time 0.98 ± 0.01 .

Cube Imitation, Picture Completion Test No. 2, and Cube Construction Tests. These were given as described by Miss Frances Gaw in *Report No. 31* of the Industrial Fatigue Research Board.

II (b). CHILDREN AND WORKPEOPLE TESTED.

Besides the above-mentioned group of school children, there were tested six groups of workpeople in three different works in Sheffield².

Conferences were held with these groups when the vocational guidance aspect of the present investigation was explained and their approval gained. Some details of the groups are as shown in Table I.

¹ *J. Nat. Inst. Indust. Psych.* 1925, II, 321.

² The writers are under a debt of gratitude to the firms of Messrs Samuel Osborn and Co. Ltd., Messrs Thos. Firth and Sons, Ltd., and Messrs Cammell, Laird and Co. Ltd, for placing facilities at their disposal and for giving whole-hearted help.

Table I. *Details of groups tested.*

Group no.	Sex	Age extremes	Mean age	Occupation	No. in group
I	Males	15-20	16	Machine file-cutters	15
II	Females	16-30	20	Ditto	19
III	Males	23-50	38	Ditto	15
IV	Females	16-32	23	Ditto	16
V	Males	14-42	21	Machine tool-makers	34
VI	Males	18-52	28	Ditto	30

III. RESULTS AND CONCLUSIONS.

1. The amount of reliance that can be placed on the results needs first to be estimated. The estimation must be fourfold. Firstly, the 'stability' of the results has to be gauged. In other words, the question has to be asked: "Would similar results be obtained if other groups of subjects were tested?" The fairness and size of the sampling are here the main considerations. Secondly, there must be known what is too vaguely called the 'reliability' of the tests. This characteristic, better termed 'consistency,' can be estimated by the correlation coefficients obtained when two forms of the same test are given to the same group of persons. Thirdly, the motivation and the attitude of the subjects affect the reliance placed upon the results. And lastly, it must be known whether the tests really measure what they set out to measure. These four points will be discussed *seriatim*.

(i) As will be seen from Table I, the numbers in the Groups I to IV were small, but they were as large as could be obtained from under the charge of any one foreman. Four groups were tested in order to compensate partly for this defect. Groups V and VI were of more adequate size. With the exception of Group IV, in which some women who were suspicious of the tests were not included, whole groups under one foreman were tested, and so the selection was due only to industrial factors.

(ii) The consistency of the tests may be judged from the data given in 2 (a) above, where each test is described. The coefficients vary from 0.98 to 0.65 and are usually greater than 0.8, which may be counted high.

(iii) Considerable time and trouble were spent in obtaining not only the goodwill, but the enthusiastic co-operation of the workpeople, a large number of whom were piece-workers, and who gave their time willingly.

(iv) The two sets of tests (see below) were designed to estimate file-cutting skill and machine tool-cutting skill respectively. Their validity may therefore be estimated by finding the correlation of the tests with the foremen's estimates. These coefficients are given in Tables II and III. It will be seen that there is considerable discrepancy between the results

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obtained from the various groups, but that certain tests have high validity in certain groups.

Table II. *Correlations between the tests and foremen's estimates.*

Test	(File-cutting.)							
	Group I		Group II		Group III		Group IV	
	<i>r</i>	P.E.	<i>r</i>	P.E.	<i>r</i>	P.E.	<i>r</i>	P.E.
Cube construction	.77	.11	.41	.14	-.02	.17	.26	.16
Aiming	.72	.12	.32	.15	-.02	.17	.19	.16
Abstraction	.62	.14	.38	.14	-.40	.14	.07	.17
Picture completion	.56	.14	.29	.15	-.42	.14	-.21	.16
Substitution	.40	.16	.52	.13	-.35	.15	-.34	.15
Cube imitation	.37	.16	.62	.12	-.51	.13	-.44	.15
Area sorting	.26	.17	.48	.14	.00	.17	—	—

Table III. *Correlations between the tests and foremen's estimates.*

Test	(Machine tool-making.)			
	Group V		Group VI	
	<i>r</i>	P.E.	<i>r</i>	P.E.
Moving line A	.71	.06	-.35	.11
Cube construction	.62	.07	.01	.12
Moving line B	.58	.08	-.06	.12
Inverse formboard	.49	.09	.13	.12
Balancing	.48	.09	.21	.12
Slide rest	.35	.10	.19	.12
Obstacle	.33	.10	.02	.12

2. It was very disconcerting, and later, illuminating, to find that these consistent and otherwise reliable tests seemed to vary in validity from group to group. Especially was it interesting to obtain such high negative correlations. These seemingly contradictory results could be caused by the combination of at least two factors. Firstly, the less the spread of ability within the group (given a constant individual variability) the less will be the correlation coefficients. The coefficients of variability showed this factor to be unimportant in Groups I to IV, but of some importance in lowering the correlations of Group V nearer to the size of those of Group VI.

Secondly, there can be the complicating factor of age. The groups varied, it has been noticed, very much in their age composition as well as in sex. Now all workmen begin their careers at approximately the same age; therefore age and experience are here nearly synonymous terms. The relationship between age and skill in file-cutting or machine tool-making was investigated. The correlations between age or experience and foremen's estimate of industrial skill were:

Group I	.98 ± .05	Group IV	.48 ± .13
Group II	.24 ± .07	Group V	.83 ± .04
Group III	.78 ± .07	Group VI	.47 ± .10

The regression line in each case is nearly rectilinear. In Fig. 3 are plotted against age of workmen, the foremen's estimates of workmen's skill in their work. From the above correlation coefficients and the curves of Fig. 3, it will be seen that, in general terms, the more experienced the workman (at any rate, up to the age of 50), the greater his skill in his occupation.

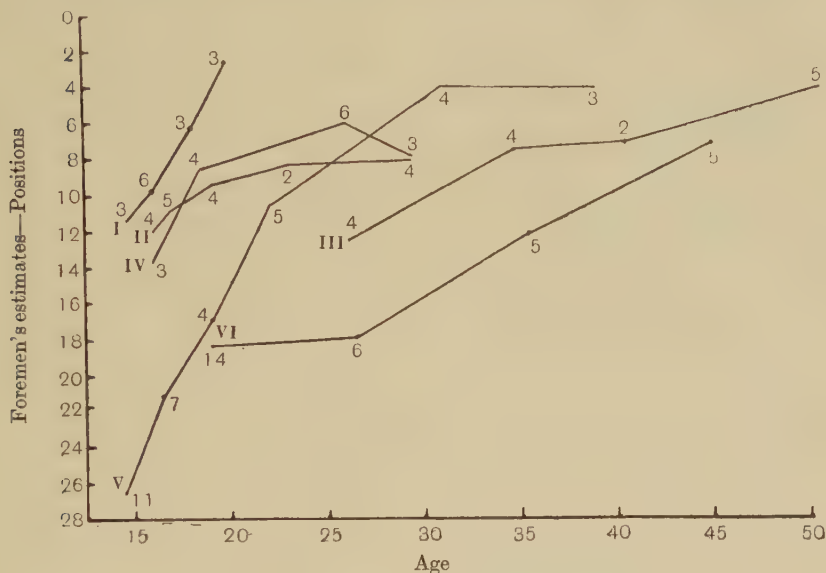


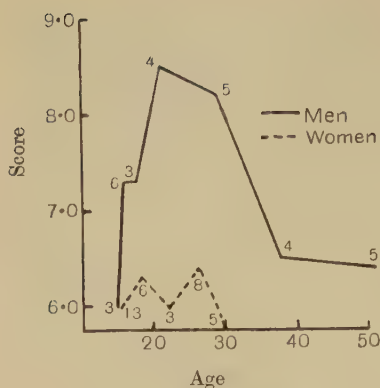
Fig. 3. Variation of age and skill in occupation.

(The small numbers on the curves give the number of cases that have been averaged to fix the points. The roman numerals indicate the group.)

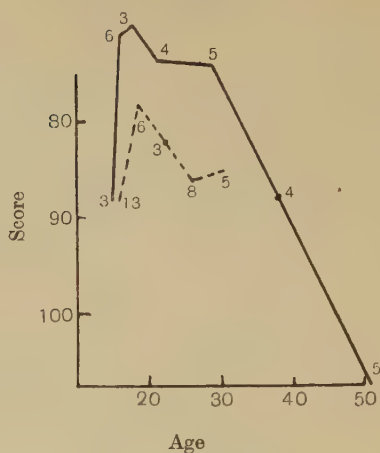
There remains, then, the question of how ability to perform the tests varied with experience. The graphs of Fig. 4 plot age of workmen in all groups against ability in each test separately. It will be seen that in every case the curve is of the same type. The test abilities increase with age up to about 25 to 30, and then decrease again to the age of 50. The fewness of the cases makes this conclusion tentative only, but it is strengthened by the fact that the phenomenon occurs in every one of the tests. It is further strengthened by the fact that each group, from no matter which of the two occupations tested, shows the same characteristic. Fig. 4 (b), Curve 11, illustrates this in the case of the Cube Construction Test.

The seemingly varying validity of the tests as witnessed by the coefficients in Tables II and III may now largely be explained by the

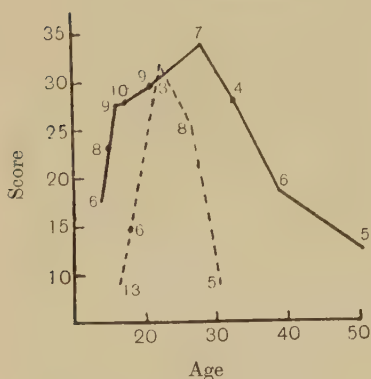
Curve 1. Cube Imitation.



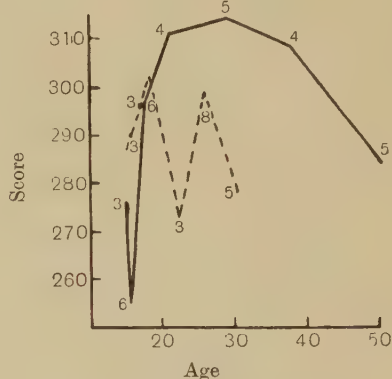
Curve 2. Substitution.



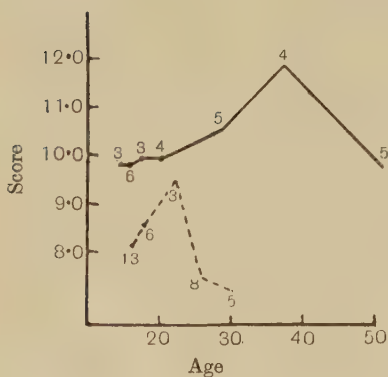
Curve 3. Abstraction.



Curve 4. Aiming.



Curve 5. Judged Reaction.



Curve 6. Picture Completion.

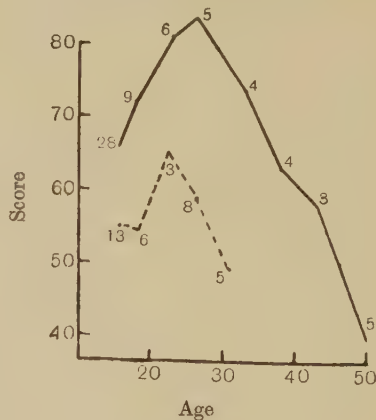
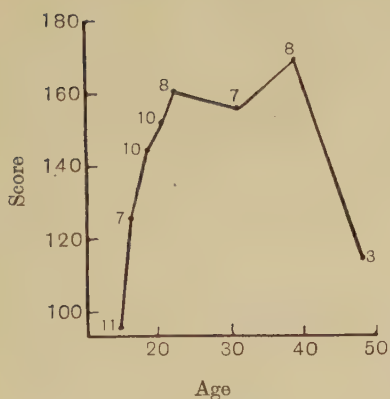
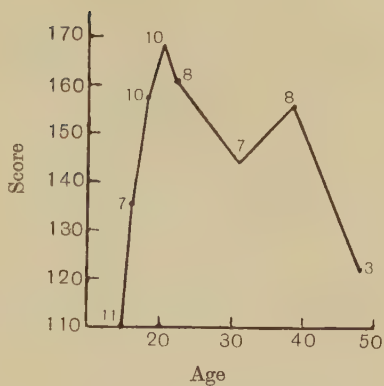


Fig. 4a. Variation of test ability with age.
(The small numbers on the curves give the number of cases that have been averaged to fix the points.)

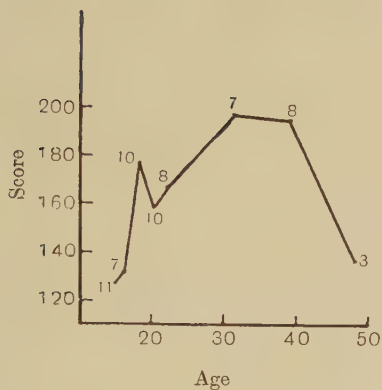
Curve 7. Obstacle.



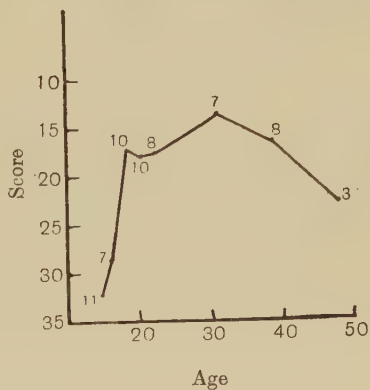
Curve 8. Inverse Formboard.



Curve 9. Balancing.



Curve 10. Moving Line A.



Curve 11. Cube Construction.

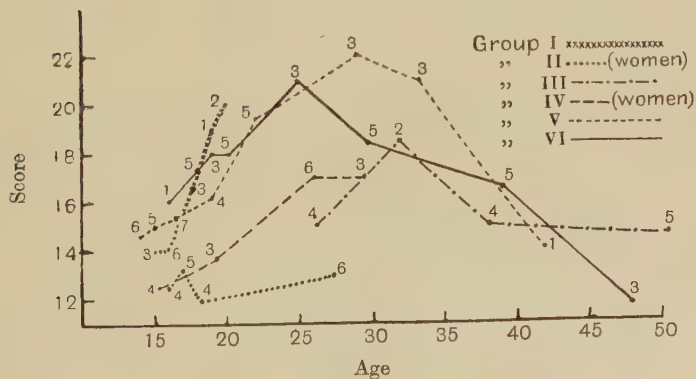


Fig. 4 b.

joint effect of (i) the curvilinear relationship shown to exist between age and ability to do the tests; (ii) the rectilinear relationship shown to exist between experience and skill in industrial work; (iii) the variability of the groups and (iv) the facts of the age composition of the six groups. Thus Group I had high positive coefficients, because all members of the group were below 25 years of age; Group II had lower positive coefficients because the group contained four persons over 25 years of age, though the effect of this was lessened by there being a low correlation between age and industrial skill in this group; the high negative coefficients of Group III were caused by there being all but one person *above* 25 years of age; in Group IV the ages were evenly distributed with their mean at 23, and consequently the mean correlation coefficient was just positive; only six were over 25 in Group V, and the average coefficient was moderately high; whereas the coefficients of Group VI were on the average nearly zero since the group was nearly evenly distributed on either side of the age 25, and the variability of the group was less than in Group V. Thus the results from each group were actually in agreement.

3. If the above conclusions with regard to test ability, age and industrial skill, be confirmed for other groups and occupations, it would have wide theoretical and practical applications.

(i) It might well be that the tests do really measure practical capacities, in which case the conclusion might be stated in the terms that whereas practical *skill* improves continuously with experience, practical *capacity* reaches a maximum and then declines. Age is seen acting as an interference factor¹.

(ii) This situation is analogous to the generally accepted fact that the higher intellectual skills increase continuously with experience, but intellectual capacity reaches its maximum fairly early in life and then declines.

(iii) The conclusion is at variance with the generally accepted and well-supported one, that intelligence in average persons reaches its height not later than 16. The usual intelligence tests are in the main verbal, and perhaps the predominant factor measured is facility and accuracy in dealing with meanings *via* words. Now none of the intelligence tests used in this investigation (viz. Cube Construction, Abstraction, Picture Completion, Inverse Formboard and Obstacle) needed for their correct practical responses—responses needing only a low degree of skill—the facile or accurate use of words. They needed merely com-

¹ Cf. J. R. Thompson, this *Journal*, 1919, x, 81-100.

prehension of meanings. The results show in these cases a progressive increase in intelligence up to 25 or 30 years of age and then a decline.

(iv) The curves give the suggestion that the maximum of intelligence might be reached somewhat before that of practical ability.

(v) A technical application of the conclusion lies in the construction of trade tests. The usual method of trying out trade test batteries on poor, medium and excellent journeymen needs to be modified. Such a selection of men needs the qualification that the journeymen should be under 25 years of age.

4. It will be noticed that the two groups of women score considerably worse than the men in the tests. Groups I (men) and II (women) worked under the same foreman, and under the same conditions. The same remark applies to Groups III (men) and IV (women). This result which, owing to paucity in numbers, is merely an indication, is in keeping with other measurements that have been made of a sex difference in practical ability.

5. To discover whether there was any group factor of practical ability manifested in the tests, the results from Group V were further treated statistically. Professor Spearman's tetrad difference method was taken as valid. The partial inter-correlation coefficients of the tests, keeping age constant, are given in Table IV. This procedure of keeping age constant was judged fair since the group had five members just over 25 and a sixth 42 years old, and the relationship between test ability and age passed Blakeman's criterion of linearity.

Table IV. *Inter-correlations of Tests (with age constant).*

	1	2	3	4	5	6	7	8	9
1. Picture completion	—	.50	.31	.62	.27	.50	.47	.15	.25
2. Moving line B	.50	—	.32	.25	.78	.23	.28	.20	.25
3. Obstacle	.31	.32	—	.51	.20	.35	.42	.34	.24
4. Cube construction	.62	.25	.51	—	.50	.13	.16	.31	.19
5. Moving line A	.27	.78	.20	.50	—	.51	.23	.14	.00
6. Balancing	.50	.23	.35	.13	.51	—	.40	.08	.14
7. Abstraction	.47	.28	.42	.16	.23	.40	—	.48	-.22
8. Inverse formboard	.15	.20	.34	.31	.14	.08	.48	—	.12
9. Slide rest	.25	.25	.24	.19	.00	.14	-.22	.12	—

The 378 tetrad differences of this Table give a histogram (Fig. 5) which is not normal. The theoretical probable error due to the operation of chance sampling errors, utilizing Spearman's formula¹, is 0.0558, whereas the actual median difference was 0.0519. The shaded portion indicates those portions of the obtained histogram which are significantly different from the theoretical values. There were 24 of these, that is,

¹ Spearman, *Abilities of Man*, Appendix, p. xi, formula 16 A.

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there were 24 tetrad differences caused by group factors. The significantly great tetrad differences were fairly evenly spread throughout the square table, except that the Inverse Formboard and the Slide Rest Tests were involved in very few, and the Moving Line A and B (which, of course, have much in common), Picture Completion, and Abstraction Tests were involved the most frequently. The actual group factors at work were not clear, although by the method of partial correlation (g constant) the indications were that the manipulative skill tests went together and that the rapid shifting of attention involved in the Slide Rest Test was an interference factor in those tests which needed steady concentration of attention.

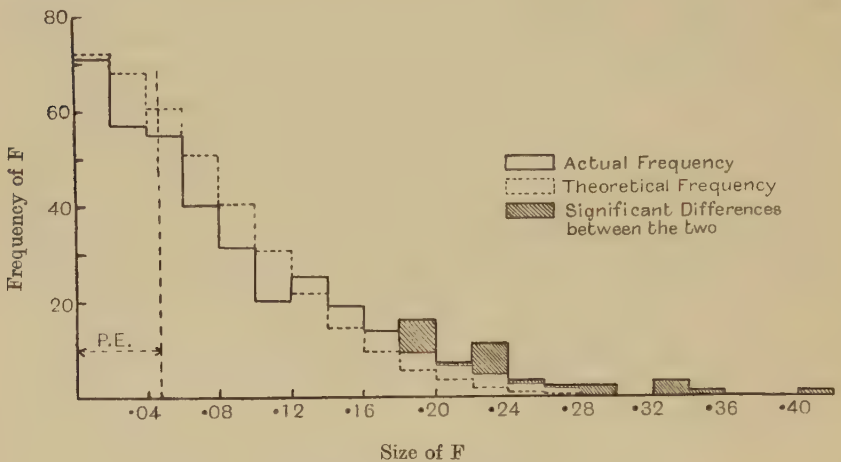


Fig. 5. Theoretical and Actual Histograms of F's.

The amount of g in some of the tests of skill was surprisingly large. For example, in the Obstacle Test there was 0.69, in Balancing 0.56, and in Moving Line B 0.52, whereas in the tests involving a negligible amount of manual skill there was only a comparable amount, *e.g.* Picture Completion 0.68, Abstraction 0.56¹.

IV. CONCLUSIONS SUMMARIZED.

1. Tests of practical abilities have been devised giving consistent results.
2. Practical ability increases up to the age of 25 to 30 and then decreases, whereas practical skill (at least in file-cutting and machine tool-making) increases progressively up to 50 years of age.

¹ Calculated by formula 20, Appendix, p. xvi of Spearman, *Abilities of Man*.

3. Intellectual ability, when tested in a practical situation, also increases to a maximum at 25 years and then decreases.
4. Age is thus shown to act as an interference factor.
5. The women scored less than the men in all the tests.
6. One or more group factors are shown to be present.

(Manuscript received 20 July, 1928.)

A SPEED-ACCURACY COMPETITION

BY W. J. STAINER.

1. PURPOSE AND PLAN OF EXPERIMENT.

A FORMER article¹ dealt with variations of rate of work in schools under ordinary conditions. The present experiment was designed to secure definite improvement in the speed and accuracy of the mechanical processes involved in arithmetic, and in the retention and reproduction of English words.

Whereas the tests described earlier were intended to measure, under normal methods of instruction and stimulus, variations in rate of work throughout a school day or term, those described in this article were designed to ascertain by statistical methods what amount of improvement could be made by the systematic use of definite tests, accompanied by various carefully selected forms of stimuli.

The arithmetical tests were made much more complicated and difficult than in the former enquiry, and others (spelling) were added, the time allotted was much increased, and the exercises were followed by time devoted to corrections, to suggestions as to methods of calculating, and to noticeable features in the spelling of certain words. In short, every effort was made to show to the full the effects of practice and adaptation exerted through a whole year.

One period per month, on a fixed day (*e.g.* the fourth Thursday) at a fixed time, was assigned to each subject, 10 minutes being allotted to the actual working of the papers, the remainder of the period to corrections.

The exact timing of the tests was arranged as in previous experiments.

The Forms were thrown together for purposes of competition, in groups of Forms, *e.g.* Sixths, Fifths, etc.

Samples of the arithmetical exercises are:

Add	6786	9473	etc.	4 points each
	9378	5947		
Subtract	6347	8234	etc.	4 points each
	3986	4795		

¹ This *Journal*, XIX, 439-451.

Multiply	5684	9538	
	9	8	etc.
	—	—	
	—	—	4 points each

Write down Quotient and Remainder:

$68 \div 9$ $86 \div 17$ etc. 2 points each

These were repeated in series, until the maximum points available amounted to between 500 and 600. No pupil, at any one of the tests, attained to the maximum points.

For the subject of spelling, lists of words frequently misspelt in the school were compiled. Some were given correctly, some incorrectly (in the common misspelling). The instructions were:

“Write (✓) against all words which are correctly spelt. Against all that are wrongly spelt put a cross (x), and then write the word correctly.” One point was awarded to each correct effort.

Different numbers of words were used for different groups of Forms, and many words were used, month after month, until they were correctly spelt by nearly all the pupils.

‘Speed-Accuracy Indexes’ were assigned to each pupil and to each Form in arithmetic and in spelling, and in the two combined the results of each test were posted in each Form-room on a sheet large enough to contain the reports for one year. Each pupil was therefore enabled to check not only his own progress, but also the progress of his Form.

The results were also cast into ‘House’ order, so that each pupil knew not only his own personal ‘Speed-Accuracy Index’ but was also informed as to the extent to which he was helping to obtain that of his Form, and of his House.

In addition to the personal stimulus afforded by ordinary types of competition, and to those due to the usual play of the ‘team’ spirit, it was thought well to add other incentives, intangible yet appreciable.

To each Form at the head of its Group on any occasion, the title of ‘The Champion Form’ of the Group was awarded, and any Form which improved its position in its Group was given an additional ‘Games period.’

A silver cup (the Speed-Accuracy House Cup), held for one year, was assigned to the House obtaining the highest index during the year.

It was distinctly understood that, other than by means of the competition, no unusual attempt was to be made either to teach spelling in an explicit manner, or to secure accuracy in ciphering, and no interference with the ordinary routine was to occur.

There is considerable reason to believe that the psychological effect of setting up an ideal of accuracy and of the regular and systematic return to the tests, coupled with the attention and interest aroused and the incentives offered to progress in this particular part of the work of the school, have been of great value.

Although it is impossible to obtain statistics regarding the relative degrees of speed and accuracy before and after the institution of the competition, those who are at work within the scheme are unanimous in their expressions of appreciation of its value.

On three occasions during a year, a private stimulus was offered to a House to make a special effort to improve its position, and it is notable that on two of the three occasions the House thus privately incited fell back from a position near the top to the bottom of the list showing the Houses in order of merit.

2. STATISTICAL RESULTS IN ARITHMETIC.

Tables I and II indicate the figures obtained from the school during one year.

Table I.

Year	Form	Average number of points scored by the boys in the Form								
		Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July
VIII	A	65.5	65.3	69.0	62.2	66.7	66.5	69.5	71.5	55.2
	B	59.2	56.2	62.3	64.7	63.3	68.8	74.3	78.0	64.0
VII	C	53.3	65.8	69.7	68.1	66.3	71.6	74.7	74.7	64.1
	D	52.8	58.5	58.2	60.1	57.3	57.3	67.1	66.1	58.8
VI	E	66.3	67.0	71.4	71.2	71.7	74.1	77.9	77.8	75.4
	F	60.2	74.9	78.7	75.8	79.3	80.6	87.3	89.2	88.3
	G	47.1	53.7	55.5	56.5	56.8	56.3	56.0	60.2	54.4
V	H	51.4	55.4	57.9	57.6	57.8	63.0	65.7	69.3	63.7
	I	53.7	54.7	60.1	58.9	60.6	62.5	67.0	70.9	67.7
	J	54.1	53.5	57.1	55.7	56.0	53.6	62.8	65.4	63.0
	K	43.5	47.6	49.3	50.4	48.3	59.5	52.3	55.0	52.6
IV	L	59.2	62.6	66.7	65.1	62.6	68.0	74.5	76.1	72.1
	M	52.5	53.4	56.4	52.4	60.1	57.0	65.9	70.6	66.2
	N	47.2	53.0	55.0	56.2	58.0	59.4	61.7	62.4	57.8
	O	47.7	52.2	54.7	54.1	55.5	51.8	58.7	59.7	60.2
	P	45.9	48.0	51.3	52.1	50.1	50.2	58.4	62.7	55.8
III	Q	54.6	62.7	66.0	60.7	61.6	64.2	69.1	72.8	69.4
	R	54.8	56.7	54.7	56.0	56.7	56.9	63.1	65.7	65.5
	S	48.0	50.0	51.7	52.6	53.7	54.7	60.7	63.0	61.5
	T	47.4	49.5	58.6	59.2	57.0	56.9	58.9	63.4	61.9
	U	42.5	47.5	50.2	53.6	53.4	54.6	58.0	59.4	59.2
II	V	51.9	59.1	66.7	66.9	73.1	77.4	79.3	88.1	83.0
	W	50.0	48.4	57.7	60.7	61.0	61.2	67.8	72.8	68.4
I	X	49.4	45.4	55.7	56.4	57.9	53.1	59.3	63.5	61.6

Table II. *Summary for year.*

Year	Aver. number taking the test	Aver. age at end of year y. m.	Average number of points scored by boys in Groups									Per- centage* of increase during year
			Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July	
VIII	17	18 7	62.9	61.5	66.2	63.2	65.3	67.4	71.5	74.2	58.8	20.7
VII	21	17 5	53.1	63.4	65.9	65.4	63.3	66.8	72.2	71.8	62.3	36.0
VI	65	16 7	57.9	65.2	68.5	67.8	69.3	70.3	73.7	75.7	72.7	30.8
V	105	15 7	50.7	52.8	56.1	55.7	55.7	59.7	62.0	65.2	61.8	28.6
IV	131	14 5	50.5	53.8	56.8	56.0	57.3	57.3	63.8	66.3	62.4	31.3
III	116	13 2	49.5	53.3	56.2	56.4	56.5	57.5	62.0	64.9	63.5	31.1
II	50	12 1	51.0	53.8	62.2	63.8	67.1	69.3	73.6	80.5	75.7	57.8
I	24	11 4	49.4	45.4	55.7	56.4	57.9	53.1	59.3	63.5	61.6	28.5

* Measured from minimum to maximum.

The statistics given in Table I, in which the grouping into years is indicated on the left, were then averaged in years. To avoid the large amount of calculation necessary to obtain them more exactly, except in the cases of the seventh and eighth years, the average figure for the Groups of Forms was taken. (The arithmetical error thus introduced is not great, since each Form contained approximately the same number of pupils.)

This appears to have the advantage of eliminating variations due to the accidental absence or presence in a particular Form of individuals possessing a markedly high or low index.

Table II gives approximately accurate statistics for each of the years of the school course.

It is noticeable that, without exception, the indexes are lower in July than in June, as was also the case, with one exception, with the individual Forms. This may be a fatigue effect, but is more probably due to distractions occurring near the end of a school year, which also immediately follow First and Second School Examinations for many of the pupils and Term Examinations for the remainder.

Another noticeable point is the remarkable approximation to uniformity in the percentage increase during the year. The exception (year II) is that of a Group consisting largely of smart young pupils whose progress was quite remarkable.

It should be noted that the arithmetic indexes are percentages of a number of points higher than any pupil could attain, greater for a higher year than for a lower, but remaining constant during the year for any particular Form.

For the eighth year, for example, the maximum number of points was 400, whilst that for the second year was 200.

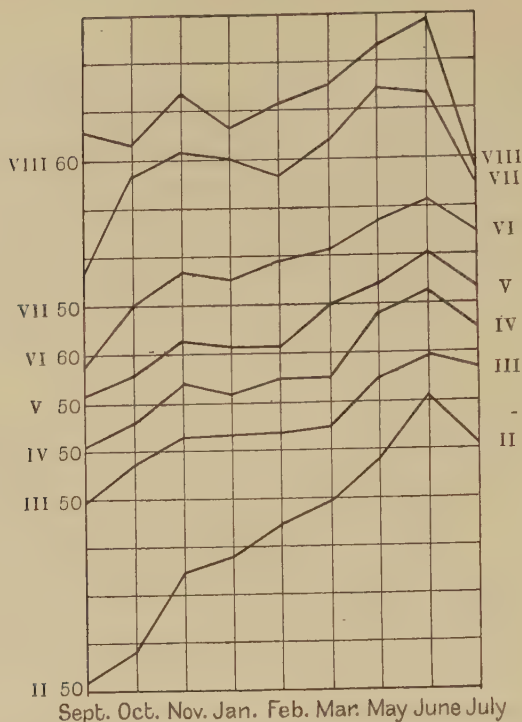


Fig. 1. Speed-Accuracy Indexes in Arithmetic. Each square represents 5 units. The unit from which the graph of each year commences is indicated on the left.

3. STATISTICAL RESULTS IN SPELLING.

Tables III and IV indicate the figures obtained from the school during one year.

Table III gives the Form statistics. The indexes for each Form are an average of the points awarded. In this case, as the words became better known during the year, a selection of more difficult words was gradually made.

Percentages were also obtained, as in the case of arithmetic, but the number of words required of each Form was gradually raised during the year, and the percentages did not therefore indicate the progress made as well as did the averages.

The use of averages does not indicate the whole progress, since the figures refer to quantities of gradually increasing order of difficulty. Whilst some amount of retardation is visible in the July statistics as compared with those of June, there is by no means the same pronounced fall as is evident in the case of arithmetic.

Table III.

		Average number of points scored by the boys in the Form								
Year	Form	Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July
VIII	A	85.6	87.3	101.7	103.8	105.8	104.0	105.0	107.0	105.0
	B	88.9	91.4	108.4	107.8	110.0	113.0	114.9	112.7	113.2
VII	C	82.6	82.9	98.3	104.8	101.7	98.4	108.5	105.8	107.0
	D	92.1	92.3	102.0	106.0	104.9	102.7	110.0	109.0	108.7
VI	E	78.6	80.7	94.8	95.9	99.5	94.3	97.0	99.6	100.5
	F	86.8	88.8	106.4	101.1	111.1	109.1	112.8	113.5	114.5
	G	68.2	74.2	81.7	85.8	85.7	86.6	95.3	93.7	96.8
V	H	66.9	68.9	81.4	82.4	85.9	83.9	90.3	89.1	92.4
	I	72.9	79.8	88.3	88.5	89.8	90.3	93.2	98.0	92.9
	J	61.6	66.7	72.7	78.5	79.3	80.9	82.9	86.6	86.7
	K	57.0	62.7	67.8	66.8	68.5	76.1	72.0	81.6	73.8
IV	L	56.2	62.7	71.1	75.3	74.6	80.4	79.5	86.2	85.7
	M	53.7	58.4	70.9	74.1	73.1	75.8	82.3	84.4	84.3
	N	48.7	55.5	64.1	65.7	70.0	70.9	80.0	82.3	79.5
	O	50.1	54.0	62.8	61.4	65.7	67.9	73.3	79.9	75.4
III	P	42.6	47.0	53.6	51.9	56.4	55.1	57.0	64.5	64.9
	Q	47.5	48.9	58.4	58.3	57.9	58.4	64.3	67.4	67.2
	R	43.2	44.7	55.6	54.3	53.6	54.7	60.0	65.8	62.7
	S	44.6	44.4	50.9	52.3	53.7	57.5	60.3	66.8	61.3
	T	32.4	36.8	51.5	42.8	47.2	41.7	45.6	46.3	49.9
II	U	35.7	42.3	49.8	49.4	54.0	59.2	60.4	65.7	64.7
	V	28.2	35.8	47.9	46.2	51.6	51.0	51.9	56.1	52.9
I	W	30.9	30.5	37.4	36.6	40.4	39.5	44.6	47.5	46.7
	X	28.9	32.0	41.3	40.4	40.8	39.4	34.4	37.5	38.7

Table IV. *Summary for year.*

Year	Aver. number taking the test	Aver. age at end of year	Average number of points scored by boys in Groups									Per- centage* of increase during year
			Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July	
VIII	17	18 7	87.0	89.0	104.5	105.4	107.5	107.7	109.1	109.3	108.4	25.6
VII	21	17 5	85.8	86.0	99.5	105.2	102.8	99.8	109.0	106.9	107.6	27.0
VI	65	16 7	77.9	81.2	94.3	94.3	98.8	96.7	101.7	102.3	103.9	33.4
V	105	15 7	64.6	69.5	77.6	79.1	80.9	82.8	84.6	88.8	86.5	37.5
IV	131	14 5	50.3	55.5	64.5	65.7	68.0	70.0	74.4	79.5	78.0	58.1
III	116	13 2	40.8	43.4	53.2	51.4	53.3	54.3	58.1	62.4	61.2	52.9
II	50	12 1	29.6	33.2	42.7	41.4	46.0	45.3	48.3	51.8	49.8	75.0
I	24	11 4	28.9	32.0	41.3	40.4	40.8	39.4	34.4	37.5	38.7	41.2

* Measured from minimum to maximum.

The case of Form X is peculiar. Several new pupils joined in May, and all obtained unusually low indexes. These have depressed the last three indexes of the Form as well as the percentage of increase during the year. For this reason graphs of the work of this Form are not included.

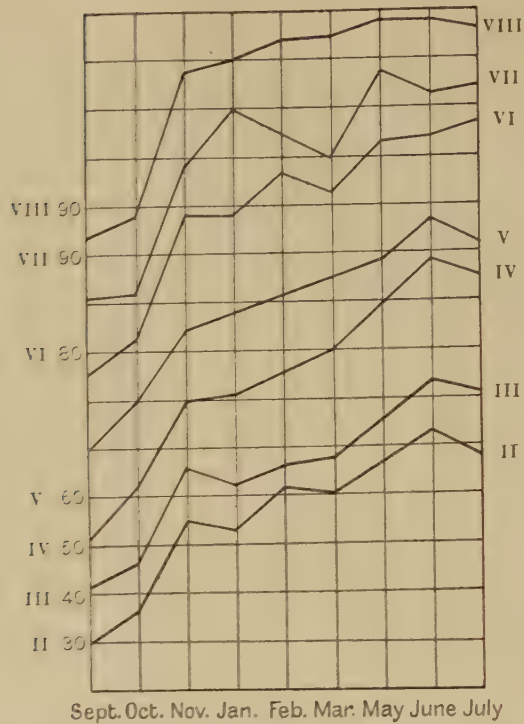


Fig. 2. Speed-Accuracy Indexes in Spelling. Each square represents 5 units. The unit from which the graph of each year commences is indicated on the left.

4. SEASONAL VARIATIONS.

A remarkable feature of the arithmetical graph, shared to a lesser extent by that of spelling, is the flattening which occurs in the months of January, February, and March. This may have been due to the attempt, previously mentioned, to apply a private stimulus to individual Houses. More probably, however, it may be due to a seasonal fluctuation in mental 'fitness' or efficiency.

It appears to be a well-established fact, although there is no solid detailed agreement among investigators, that both mental and physical efficiency fluctuate more or less rhythmically throughout a day and a year.

"For memory work and concentration of attention, the most favourable period is from October to January; then a decrease of mental power sets in, continuing to June and rising again from June onwards. Temperature and mental power appear to vary inversely¹."

¹ R. R. Rusk, *Experimental Education*, p. 248.

It would not, of course, be correct to say that the graphs of Figs. 1 and 2 confirm this statement in every particular. Having regard, however, to the fact that the quantities measured represent knowledge, skill, and alertness, and that from month to month these should increase, apart altogether from fluctuations in mental energy observable throughout a Term¹, the graphs would appear to indicate quite clearly that mental energy is greater in autumn and summer than in spring.

¹ This *Journal*, XIX, 448-451.

(*Manuscript received 6 December, 1928.*)

PUBLICATIONS RECENTLY RECEIVED.

Practice, Fatigue and Oscillation. A study of work at high pressure. By J. C. FLÜGEL, B.A. *The British Journal of Psychology Monog. Suppl. XIII.* Pp. 92. 8s. 6d. net.

This is the record of an investigation by Mr Flügel in 1922 on 46 school children working at Kraepelin's adding sheets for 20 minutes on each of 46 successive week days. Among the principal conclusions were the following:

(1) The ability of the subjects showed steady improvement even toward the end of the experiment, and the usual phenomenon of a plateau was not discovered in their curves of work. The chief reason for this would appear to be that the increasing monetary rewards that were given to the children every time they broke their own records more than counter-balanced the monotony of the work.

(2) There was a correlation of 0.51 between initial ability and improvability.

(3) The more able subjects showed more variability from day to day than the less able.

(4) There was a considerable difference between individuals as regards fatigability, the more able individuals being on the whole less fatigable than the less able.

Unfortunately no conclusions could be drawn as to a natural periodicity in output. In order to make possible the system of marking, a special signal was given at the end of each minute and a special sign had to be written by the subjects in their books at the moment when the signal was given. The consequence was that the curves of work showed an artificial rhythm of more or less similar character in consonance with the minute signals, instead of a natural rhythm, different in each case. To obtain curves showing natural periodicity a series of experiments on slightly different lines would be necessary, but the present work is of great value, especially in demonstrating the effect of a system of incentives on the shape of the work curve.

Science and the Religious Life. A Psycho-Physiological Approach. By CARL RAHN. Yale University Press (and the Oxford University Press). 1928. Pp. x + 221. 13s. 6d. net.

This short book upon an enormous subject is unusual and noteworthy in some respects. It is not an attempt to explain religion in terms of psychology or physiology, as might be thought from the title, or an attempt to defend it against their encroachments. In fact, the author never puts himself to the trouble of giving any convincing proofs of the relation that he takes to exist between Science and Religion. He expresses his views, but does not state his reasons. This gives me access to the two criticisms that I wish to make. The real value in the book appears to be that it is a review of the present position of science, or, as he calls it, Naturalism, in order to try to judge where, and in what way, the pursuit of it is leading men. Mr Rahn appears to be afraid that contemporary Naturalism is failing to fulfil its promise to set up new standards in the spirit of inquiry, and hence in intellectual and moral freedom as well. His suggestions as to the possible nature and origin of this failure are very interesting, but in general his critique is not penetrating enough. As a consequence, however, of his view, which is never proved to be tenable, that physiology can explain all psychological phenomena, he claims that Naturalism (that is to say, physiology in this particular connection) must be a thoroughly inspiring ideal, since it can explain to us the biological value of religious experiences. If it is agreed to refrain from asking Mr Rahn to establish the soundness of his philosophy, the book remains an interesting sketch of some current problems.

The Symbolic Process and its Integration in Children. A Study in Social Psychology. By JOHN F. MARKEY, Ph.D. London: Kegan Paul. 1928. Pp. xii + 192. 10s. 6d. net.

The book contains a study of the nature and mode of development of word symbols in the individual; a short chapter, well worth reading, on the "delayed reaction"; and a clear exposition of the "stimulus-response" formulation when carried to its extreme position, including the view that "meaning" exists purely in the reaction. This is provoking in proportion as it is unconvincing.

Judgment and Reasoning in the Child. By Professor JEAN PIAGET. London: Kegan Paul. 1928. Pp. viii + 260. 10s. 6d. net.

It is very interesting to have a sequel to Professor Piaget's most valuable book, *Language and Thought of the Child*. Perhaps the greatest fault of the present book is that, while it contains an astonishing mass of carefully collected and analysed data, nowhere in his work does the author propound his conclusions in the shape of a simple and logical reconstruction. It is at the same time absolutely necessary and yet most difficult to make a reconstructive abstract of the work as one goes along, and if such an abstract is to be easily followed and apprehended it must be simple and logical. In this respect Professor Piaget has committed the error to which all true scientists are liable; he has allowed his ideas to float away into an ocean of facts. Thus, it is difficult to express the general contention of the work except in some such dry and paltry phrase as "the child does not think in the same way as the adult thinks." It seems that Professor Piaget need hardly have written two large books in order to discover anything so simple. Yet this is hardly fair.

A rapid review of the concluding chapter shows that the child, at the age at which Professor Piaget is interested in children, is neither perfectly play-like in his vocal activities, nor perfectly intelligible to an outsider in accordance with the standards of adult thought. He is, in fact, "ego-centric." This means a lack of conscious insight: he is not able to manipulate his thoughts in such a manner that each step and its relations to the other steps are well-defined, nor is he able to return on the path of his argument. Together with this there goes an inability to use logic—that is, the causal relatedness of thoughts—and hence a tendency to think in terms of accidental juxtaposition. This leads to a weakness of synthetic ability, for the thoughts are not balanced out according to their true relative values, but are thrown together anyhow. All this is really connoted by the term "syncretism," and the phrase "insensibility to contradiction." Professor Piaget speaks of the child as having an "intellectual realism," which is different both from an "intellectualism" and a "realism." This is difficult to comprehend, and yet it may be one of the most illuminating things he has said. But one feels that children must be much more variable than he supposes—perhaps even as variable as adults—in this respect. His final conclusion, that "causality," as understood by the trained and adult scientist, is not employed among the intellectual instruments of the child, is almost indisputable.

Instinct and Personality. By A. C. GARNETT. London: George Allen and Unwin. 1928. Pp. x + 218. 8s. 6d.

A. C. Garnett gives a good simple presentation and criticism of the theories of McDougall, Coué and the psychoanalysts, which he tries to synthesize into a complete dynamic theory of the organism. He holds that meaning is the guiding factor in any activity and that this consists essentially in "expectation"; thus "the forward looking teleological consciousness is the ultimate fact of existence"; expectation becomes the *hormé* or urge of life. The consideration of play and ideals shows that instincts cannot be regarded as the sole springs of action; moreover, the accumulation of instinctive energy is an untenable assumption. The author is also led to the difficult position of

maintaining that all so-called unconscious mental processes are "purely physiological continuations of neural processes initiated by expectant attention." Various psychological data are reviewed in this light and a general monistic philosophy advanced.

The attempt is certainly suggestive, but difficult to evaluate. Expectation is a curious concept on which to build and one that must give rise to some confusion.

The reviewer would like to add that he has seldom come across a psychological book with so pleasant a format and type.

The Guidance of Conduct. By E. T. DIXON. London: Kegan Paul, Trench, Trubner and Co. Psyche Monographs, No. 2. 1928. Pp. ix + 219. 10s. 6d.

Despite its title this book is not another popular exposition of psychology applied to everyday life. From a logico-philosophical rather than from a psychological point of view the author urges that the sensations and ideas of associationism and traditional philosophy should be replaced by "formulae" whose function is to guide reactions. Thus words and images such as Value should not be hypostatized into entities that are assumed to represent realities, they are merely formulae symbolic of certain types of behaviour. Dixon's formulae are, of course, closely similar to the patterns of contemporary psychologists. This pragmatic theory is developed in relation to ethics, sociology and economics.

Types of Men: The Psychology and Ethics of Personality. By EDUARD SPRANGER. Translated from the fifth German edition by P. J. W. PIGORS. Halle: Max Niemeyer. 1928. Pp. xii + 402. R.M. 15.00.

The use of the notion of types seems to be returning to psychology in many different directions. Undoubtedly this is due, on the one hand to the revolt against the piecemeal, bit by bit study of human reactions, and on the other to an increasing attempt to deal psychologically with the higher mental functions. Especially in Germany "characterology" threatens to become the new psychological fashion, and numerous books purporting to discuss this topic in a scientific manner have appeared during the last few years and have met wide popular approval. Naturally these books are of very varying value, but Professor Spranger's is beyond doubt one of the most stimulating and valuable. To the more exclusively psychologically trained reader Spranger's outlook and work will appear far more philosophical than psychological, but his wide knowledge and his keen judgment constantly afford material of the greatest social and psychological interest. The book contains four parts: Philosophical Basis; The Ideally Basic Types of Individuality; Consequences for Ethics and The Understanding of Mental Structures. The second and fourth parts particularly contain a mass of acute psychological argumentation and observation. Most readers may wish often that a little rigorous condensation had been effected, but the book is in many ways a rich one, and will repay study, especially on the part of the social psychologist. The translation is good.

The "Soul" of the Primitive. By L. LÉVY-BRUHL. Translated by LILIAN A. CLARKE. London: Geo. Allen and Unwin, Ltd. 1928. Pp. 351. 12s. 6d. net.

In this book Lévy-Bruhl applies his already formulated ideas concerning primitive logic to the "pre-notions" which he finds primitive people to possess of "their life-principle, their soul, and their personality." No new guiding principles of interpretation emerge, but numerous new illustrations are given within the realms dealt with.

The Child in Primitive Society. By N. MILLER. London: Kegan Paul. 1928. Pp. 307. 12s. 6d. net.

This is a well-documented study of the various ways in which the life of the child is directly shaped by social influences in a large variety of primitive social groups. It belongs to the type of book that throws a very wide net for illustrations, but all the sources are given and the treatment is perfectly clear. Not itself markedly original, it is the result of a great amount of painstaking study and will be of considerable value to the social psychologist who is in search of material.

Introduction to Social Psychology: Mind in Society. By R. MUKERJEE and N. N. SEN-GUPTA. Introduction by R. M. YERKES. London: D. C. Heath and Co. 1928. Pp. xv + 304. 7s. 6d. net.

This is a courageous and comprehensive introduction to the study of social psychology. The authors have made a genuine and in many ways a successful attempt to say where the real problems lie, and they have indicated lines of solution which any understanding reader may attempt himself to follow up. Fortunately, they have no cut-and-dried systematic schemes to offer. In fact the book is what it pretends to be, and students approaching this topic will find it a far more satisfactory introduction than most of the existing texts.

Emotion and Delinquency: A Clinical Study of five hundred Criminals in the Making. By L. GRIMBERG. London: Kegan Paul. Library of Educational Psychology, London. 1928. Pp. ix + 147. 7s. 6d. net.

This is a character study of a considerable number of delinquent girls, carried out at the New York Neurological Institute, and presented in a very readable way, with a fair collection of case histories. The girls studied appeared to fall into two groups. In the first were girls with no obvious adequate reasons for their delinquency. In general this group consisted of individuals who were mentally backward or dull, docile during early childhood, but ill-adapted to their home environment, and very often markedly selfish and liable to outbursts of temper. As a rule some special incident could be found which first definitely led to immoral or anti-legal action. In most cases there was complete absence of sympathetic religious training. The author is very definite that the downfall of these girls is rarely due to economic conditions. In the second group were girls morally completely unadapted to their society. Here, as contrasted with the first group, heredity appeared an important factor, pathological lying was very common. In general intelligence was low. Their occupational history showed a constant change of job. There was generally violent opposition between child and parents, and as a rule the career of delinquency was embarked upon earlier and before the home life was left. Girls in this group were almost always of marked and unstable emotionality, and their delinquency was mainly the expression of impulsive acts uncontrolled by cognitive factors. The author believes, and states his evidence clearly, that there is a definite "organic basis responsible for their condition." With the second group, as with the first, economic conditions were of minor importance.

The whole study is of great interest and should be followed up by an investigation, from a similar point of view, of possible remedial measures.

Emotions of Normal People. By W. M. MARSTON. London: Kegan Paul. 1928. Pp. xii + 405. International Library of Psychology, Philosophy and Scientific Method. 18s. net.

This book is undoubtedly a good deal longer than it need have been. At the same time it is written in a lively and interesting fashion, though the frequent colloquialisms may annoy some readers. It begins with an elaborate theory of consciousness, called

"the psychotonic theory." Then, in the light of this, it discusses the principles of primary emotions, which are, according to the author: compliance, dominance, inducement and submission. The rest of the book is a more detailed study of these primary emotions, their mechanisms of expression, their functions, and, to a large extent, their characteristic differences in men and women. Much of this discussion is very clever and marks a genuine contribution to the subject. The author marshals his evidence well and deals with a mass of material that has been collected at first hand. The early part of the book is certainly open to the criticism of relapsing somewhat easily into a kind of speculative physiology.

Contributions to the Principles of Morphology. By W. B. CROW. London: Kegan Paul. 1929. Pp. viii + 94. 5s. net.

Dr Crow has produced a very clear and judicious contribution towards the study of scientific method. As he says, morphological and physiological methods are supplementary, though neither can be reduced to the other. The essay, after a general sketch of the development of morphological ideas and a discussion of the relations between observation and theory, deals with some of the leading ideas in the biological study of structure. The topics considered are: individuality, homology, symmetry and proportion and relationship by descent. Though the work does not bear directly upon specific psychological problems, a psychologist may learn much from its discussion of these leading notions.

The A.B.C. of Psychology. By C. K. OGDEN. London: Kegan Paul. 1929. Pp. x + 279. 4s. 6d. net.

After all, an alphabet is a late product of culture. It may reasonably be doubted whether psychology yet possesses one, or is likely to do so for some long time yet to come. However, the exigencies of a series may perhaps be held legitimately to determine titles. Even so, it is a little difficult to see why an A.B.C. should plunge the reader almost straight off into seven different theories of the relation of body and mind. The seventh theory, which appears to be the author's own, is called the "double language theory" and the main advantage of a linguistic solution would seem to be that it is less likely than the others to offend anybody. After this, however, the book proceeds rather well, and is clear, definite and well-informed. Too much is included, and not a little, as in the discussion of "asymmetry," could not, with the widest possible stretching of tolerance, be regarded as soundly established. But the reader will learn a lot and may be stimulated to try to discover more. A bibliography is added. The bulk of the books mentioned are those of a single series, but as most of them are very good indeed, that, perhaps, does not matter.

The Psychology of Language. By W. B. PILLSBURY and C. L. MEADER. New York: D. Appleton and Co. 1928. Pp. vii + 306. 10s. 6d. net.

Nobody who wishes to learn something about either the physiological or psychological factors involved in the production and use of language could do better than read carefully this sober and comprehensive book. After a study of the general scope of the problems involved, the authors give a clear and adequate account of the mechanism of speech production, and a most admirable discussion of the psychology of language. Recent experimental work is excellently dealt with. The final two sections, dealing with sound changes in speech and with syntax, are perhaps particularly noteworthy. Well selected references for further study are given at the close of every chapter.

Crossroads in the Mind of Man: A Study of Differentiable Mental Abilities. By TRUMAN L. KELLEY. Stanford University Press, California. 1928. Pp. vii + 238. \$4.00.

Dr Kelley has here attempted an elaborate statistical study directed to the discovery and exposition of a method of proving the independence of mental traits. He

argues, with much persuasiveness, that this kind of a study is the basis of a true understanding of human nature, and that it must precede the further study of how independent mental traits function in real life. His general position is well illustrated in the following quotation: "Mental life does not operate in a plain but in a network of canals. Though each canal may have indefinite limits in length and depth, it does not in width; though each mental trait may grow and become more and more subtle, it does not lose its character and discreteness from other traits." This thesis is pursued with unwearying skill through a mass of data statistically treated. An extensive bibliography is added.

The Soul Comes Back. By J. H. COFFYN. New York: The Macmillan Company. 1929. Pp. 207. 8s. 6d. net.

Professor Coffyn argues colloquially and vigorously against modern mechanistic and materialistic theories concerning human nature and destiny. His book is, as he frankly states, a venture of faith, and it may well appeal to the reader of robust moral activity.

The Varieties of Religious Experience. By WILLIAM JAMES. London: Longmans, Green and Co. 1928. Pp. vi + 534. 6s. net.

The cheap edition of William James' classical work should receive a very warm welcome. The book is extremely well printed and produced.

Whither Mankind? A Panorama of Modern Civilisation. Edited by CHARLES A. BEARD. London: Longmans, Green and Co. 1928. Pp. vii + 408. 12s. 6d. net.

In spite of a certain flamboyancy both in the style of production and in the initial claims made by the editor for this volume, it remains a collection of stimulating essays on various special questions in contemporary social life. After a general introduction by the editor, the topics dealt with are: The Civilisations of the East and West, by Hu Shi; Ancient and Mediaeval Civilisations, by Hendrik van Loon; Science, by Bertrand Russell; Business, by Julius Klein; Labour, by Beatrice and Sidney Webb; Law and Government, by H. L. McBain; War and Peace, by Emil Ludwig; Health, by C. E. A. Winslow; The Family, by Havelock Ellis; Race and Civilisation, by George A. Dorsey; Religion, by J. H. Robinson; The Arts, by Lewis Mumford; Philosophy, by John Dewey; Play, by Stuart Chase; Education, by Everett Dean Martin; and Literature, by Carl Van Doren. The editor contributes an epilogue. As will be seen from this list, the bulk of the book is by writers who can express themselves in an extremely competent journalistic style. This they succeed in doing, and naturally enough, though the essays differ greatly in value, many of them will most effectively challenge the thought and stimulate the investigations of the social psychologist.

Gotama the Man. By C. A. F. RHYS DAVIDS. London: Luzac and Co. 1928. Pp. 302. 4s.

In this book, which is cast into the form of an autobiography, Mrs Rhys Davids tells the story and unfolds the views of the founder of Buddhism. It is all written very simply and clearly, and reveals a most persuasive and attractive personality.

The New Psychology of the Unconscious. By C. W. VALENTINE. London: Christophers. 1928. Pp. xiv + 162. 4s. 6d. net.

Professor Valentine's admirable little book entitled *Dreams and the Unconscious* having been out of print for some time, he has reissued it under this new title. It remains as before, a restrained and acute presentation of some of the doctrines for which Freud was originally responsible, mainly in their relation to problems of normal psychology. The whole has been revised, and additional notes and illustrations have

been added. The final chapter of this work is new. It deals with *Normal Individuals and the Influence of the Unconscious*, and takes up in particular the questions of repression in forgetting and dissociation in normal people. The book is intended for the general reader and is undeniably clear as well as just. It remains somewhat slight, and might well form the prelude to a more thorough study of psychoanalytic doctrines from the same point of view. It is much to be hoped that some day Professor Valentine himself will produce such a study, since this book is sufficient to show clearly that he can view exciting theories with a sane and balanced judgment.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

- May 11th, 1929. "Is there a common factor of sensorial rentivity and is it influenced by conation?" By Miss E. H. WALTERS, B.A.
- " " "A balance showing the relation of blood distribution to mental activity" (with demonstration). By S. E. W. TAYLOR, M.A.
- " " "Some factors involved in the performance of simple routine tests." By J. W. CAUGHEY, B.A.

SECTIONAL MEETINGS.

MEDICAL.

- March 27th, 1929. "Compulsive Thinking as a Castration Equivalent." By Dr GRAHAM HOWE.
- April 9th, 1929. A Symposium opened by Dr E. JONES and Dr HENRY YELLOWLEES on "The Role of Anxiety in the Psychoses and the Psychoneuroses."
- May 22nd, 1929. "The Schizoid Personality—A Biological Variant." By Dr EMANUEL MILLER.

EDUCATION.

- May 6th, 1929. "Is there a Psychology for Modern Education." By R. B. RAUP, Esq., Ph.D.
- June 3rd, 1929. "Individual 'Difficult' Children." By Miss E. M. TERRY.

INDUSTRIAL.

- April 26th, 1929. "Evidence of a Central Factor in Test of Manual Dexterity." By Mr J. N. LANGDON, B.Sc.
- June 17th, 1929. "Some Psychological Problems in Gold Mining." By Dr G. H. MILES.

AESTHETICS.

- April 25th, 1929. "The Work of an Aesthetics Group." By Miss BODKIN, M.A.